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OF THE

Royal Army Medical Corps

EDITED BY

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Original Communications.

ENTERIC FEVERS IN THE BRITISH EXPEDITIONARY FORCE.[1]

BY MAJOR-GENERAL SIR WILLIAM B. LEISHMAN, K.C.M.G., C.B., F.R.S., F.R.F.P.S.G.

I HAVE, in the first place, to thank the Royal Faculty, with great sincerity, both for the honour they have done me in appointing me to deliver the Finlayson Lecture and for their indulgence in permitting me, on more than one occasion, to postpone it, for reasons beyond my own control.

It is with great pleasure that I feel myself associated in this manner with the memory of a Glasgow physician held in such high respect and affection as the late Dr. Finlayson. I know well with what warm regard my father considered him, and I am glad to say that I myself have a very clear memory of him and of his invariable kindness and ready help for those who were embarking in the profession, of which he furnished in his own person an ideal and an example. Although I do not think I met him more than once or twice subsequent to my leaving the University and the Western Infirmary, I am glad to say that I have still the copy of his well-known "Clinical Manual" which he gave to my father, and I am always struck with the wealth of knowledge and experience which it contained, so much of which still appears, at any rate in the eyes of a pathologist, to be as wise counsel as in the day on which it was written.

In the selection of a subject which might be considered suitable I have been in some difficulty. The war put an abrupt stop to my own laboratory investigations, and circumstances have prevented my resuming them since my return from France in 1918. In choosing that of "Enteric Fevers in the British Expeditionary Force," I have been partly influenced by the fact

[1] Reprinted from the *Glasgow Medical Journal*, February, 1921.

that I had the pleasure of lecturing some years before the war to the Glasgow Medico-Chirurgical Society on "Typhoid Inoculation," and I trust I am not wrong in thinking that there may be some here who would be interested to learn how this procedure has borne the stern test of modern war.

It is a good many years since I commenced at Netley work on typhoid vaccine under my good friend, Sir A. E. Wright, and, from the time that the fates decreed that I should assume his mantle as Professor of Pathology at the Royal Army Medical College, this subject and the general problem of the protection of our Army against enteric fever in peace and war have formed a very large part of my work. A long series of reports and papers by myself and my colleagues has placed on record the results of our laboratory researches, as well as of the elaborate statistical test of the vaccine finally selected for general use. I do not propose to refer to this work further than to point out that we were eventually successful in attaining the object towards which we had deliberately worked, namely, clear statistical proof of the protective effect of the vaccine. With this in our hands we were able, in 1909, to convince our own chiefs, and then the Army Council, that typhoid inoculation was a safe and useful procedure, and permission was accordingly re-granted to its general employment in our troops—always on a voluntary footing.

As was natural, and indeed our bounden duty, we gave continual and anxious thought to the application of the method in the Army in the event of a great war. The gravity and the urgency of this danger were at no time under-estimated, at all events in the Army Medical Department. With this in view we had, for example, several years before the war, made all arrangements for the maintenance of a reserve of vaccine large enough for the first needs of an Expeditionary Force, and had worked out in detail the scheme for a great expansion of its manufacture, if this should be called for. Again, experience and military knowledge had shown us that, if inoculation should suddenly be desired on a large scale on a hurried mobilization, we should need to have in our possession accurate information as to the average degree of disability produced by each dose of vaccine, the extent to which this might interfere with the soldier's physical capacity, and the duration of such interference. We had also to know the percentage of men who might be expected to be unfit for full work 24, 48 and 72 hours after inoculation. These points and others were accordingly determined by means of a series of experimental observations carried out at Aldershot about two years before the war, and when the moment came, in August, 1914, we were able to give definite replies on such points to the very natural questions raised at once by staff officers anxious lest the inoculations should interfere with the smooth working of their mobilization time-tables.

On the outbreak of war the Director-General, Sir Arthur Sloggett, at once approached the Commander-in-Chief, Lord Kitchener, and the permission of the latter and of the Army Council was obtained for the

carrying out of as many inoculations among the men of the Expeditionary Force as might be practicable, prior to embarkation. Lord Kitchener was also good enough to circulate an order that he hoped as many men as possible would come forward for inoculation.

We had long decided that in such an event it would be altogether impracticable to carry out the customary two inoculations, at ten days' interval, and had determined, from experiment, that the best protection that could be expected under the circumstances was to be got by a single dose of 1,000 million bacteria—in substitution for the usual initial dose of 500 million, followed by a second of 1,000 million. The tests to which I have just referred as to the average degree of disability consequent on the inoculations were conducted on this basis. This single dose method was accordingly adopted during the first weeks of mobilization, full instructions were at once circulated to every command and unit concerned, and the machinery for the prompt requisitioning and dispatch of the vaccine was set in motion.

I need do no more than allude to the heavy handicap under which our medical department laboured, and still labours, in lacking compulsory powers in this matter—powers conceded before or during the war to the medical department of the American, French, Italian, and German armies. Bearing this in mind, it will, I think, be considered no mean achievement that we were able to inoculate with a single dose about twenty-five to thirty per cent of the original Expeditionary Force before they crossed the Channel, and that it was not long before the inoculation strength of our troops in France rose to a figure which fluctuated between ninety and ninety-eight per cent.

The history of enteric fever during the past great war has so many sides that I must perforce make an arbitrary selection of such of them as may be likely to interest you most. A fuller account, with the statistical and other information from which it is built up, will appear in the "Official Medical History of the War." Of these sides, prevention, from the military point of view, takes undoubted precedence, and will be dealt with first, my remarks being naturally limited to those aspects of the subject which fell within my province as a bacteriologist or, officially, as Director of Pathology of the British Expeditionary Force. I am not competent to discuss the subject of prevention from the more general side of preventive medicine, or to comment on the organization and effects of the hygienic measures devised and carried out by the Army, but I am at least free to express my admiration for the splendid work performed by my colleagues of the Sanitary Branch, and to record my opinion that never in military medical history has sanitation achieved such brilliant well-deserved success. The very narrow bounds within which enteric affections were kept in France were undoubtedly largely consequent on the care and thoroughness with which the general measures relating to drinking-water, food protection, flies, and a host of other details, were carried out, and it would, to my mind, be

both an unnecessary and an invidious task to attempt to hold the scales and assess the respective shares in the credit attributable to general sanitation on the one hand and preventive inoculation on the other. Both are essential, and neither, to my thinking, can with safety be relied upon by itself in the field.

The Results of Inoculation.

I have already mentioned the state of the original Expeditionary Force as regards inoculation on their embarkation for France; let us now follow events further in this connexion and in its bearings upon the incidence of the enteric fevers. I need hardly remind you that the military conditions consequent upon the early battles of the war and the retreat to the Marne were such as made any attempt at increasing the percentage of inoculation altogether out of the question. It was not until the fighting on the Aisne had been in progress for some time, and until the Army had been able to take breath and make progress in its reconstruction and the incorporation of its reinforcements, that it became possible to press for and to carry out fresh inoculations on any considerable scale.

By this time, towards the beginning of October, 1914, the urgent need for this became manifest to those of us whose duty it was to keep our fingers on the epidemic pulse of the Army. True typhoid had made its appearance, was showing an alarming tendency to spread in certain units, and was presenting itself, only too often, in the grave form so familiar to us in India during pre-inoculation days, and in the South African War. There is little to be gained by attempting to trace out its origins. It is true that some of the earliest cases we saw were in German prisoners, but the disease was also present among our Allies and close neighbours, the French, both in their fighting forces and in the civil population of the villages in which our men had been billeted. A case or two also joined our force from England in the incubation stage of the disease, while the laws of probability, and later demonstrable proof, made it certain that undetected carriers were in our midst. Nothing, then, stood between us and a violent conflagration except general preventive measures and the special prevention conferred by inoculation.

Permission was obtained for such inoculation to be carried out, as opportunity offered, and from that time strenuous and unremitting efforts commenced by means of lectures, etc., to persuade the men to be inoculated. Fortunately, we had in this a fairly easy task, our position as regards the arguments and proofs we were able to bring forward being far more satisfactory than was the case formerly, and it is pleasant to be able to record the strong and almost invariable support that we received from our combatant colleagues, from the Commander-in-Chief downwards. The percentage of the inoculated rose with remarkable activity, influenced naturally to a large extent by the fact that the reinforcements reaching us from home and elsewhere presented an increasingly high inoculation strength.

The effects on the threatened epidemic were most pronounced, as may be seen from some of the tables and curves which I show you. As regards these tables, I would point out that they are all taken from my own note-books, and although I accept full personal responsibility for their accuracy, they do not and cannot correspond with the final official figures which are framed on a different plan, and do not cover the same periods as those now shown. It would be impossible within the compass of a single lecture to present to you, with the necessary explanatory comment, the statistical results of inoculation in the whole of our forces throughout the war, and those in question relate solely to the British forces in France, and have been selected in illustration of our experiences of enteric in that battle area.

Before studying the figures we must briefly consider some essential points relative to statistics of this nature. It has been my fate to be largely concerned with the preparation and presentation of the earlier statistical results of inoculation, and I may perhaps claim to be familiar with at least the majority of the pitfalls and possible fallacies which such statistics may present. In this case I shall attempt no more than an indication of some of the special difficulties which we either anticipated or encountered in the campaign, and of the precautions which we took in our endeavours to combat these.

I have always been of the opinion that quite the most essential factor in the material which furnishes the matter for this form of statistical analysis is that of accurate diagnosis. I could, if need be, recall numerous instances in which doubts under this head have subsequently arisen and have either greatly weakened or completely invalidated a group of figures otherwise apparently clear and free from fallacy. This applies naturally to all medical statistics, but I think with especial force to the group of continued fevers which own so many symptoms in common. With this in mind, we took all possible care in the British Expeditionary Force to ensure such accuracy of diagnosis, and I propose to devote a few minutes to describing our procedure. The ideal at which we aimed was that every case of suspected enteric fever should be submitted to the approved laboratory diagnostic tests over a period of weeks sufficient to enable its final precise definition as either a typhoid, a paratyyhoid "A," or a para-typhoid "B" infection; or, alternatively, that it should be diagnosed definitely from positive or negative evidence as an affection of some nature other than enteric. To this end it was directed that all cases in their early days should be reported and classed, provisionally, as "enteric suspects," and the final diagnosis was only expected and accepted when bacteriological evidence of a sufficiently clear nature had accumulated as the result of a series of laboratory tests.

This plan worked well, though, as may well be imagined, it made heavy calls upon the officers in charge of mobile laboratories and on the hospital pathologists, in particular those of our isolation hospitals at Wimereux,

Etaples, Havre, and Rouen. Without exception, these officers, realizing its importance, carried out this laborious work without sparing themselves, devoting long hours to the attempted isolation of the infecting bacilli and to the labour of numerous series of agglutination observations. I am sure that never before in war has this class of patient been so thoroughly investigated, nor such meticulous care been taken to ensure accuracy of diagnosis, as was the case throughout our operations in France and Flanders. I might add that this had much more than an academic importance, since it was only by work of this class that we could obtain reasonable certainty as to the existence or non-existence of the carrier condition, and so protect the Army from the great danger of the too early return to the active ranks of a convalescent who might still be excreting Eberth's bacillus or one of the paratyphoids.

Throughout the war it was my duty to take many responsibilities and to give decisions in connexion with technical difficulties as they arose, and I should indeed be ungrateful if I did not acknowledge, most warmly, the generous way in which my brother officers accepted rulings on matters in which their own views might have differed from mine. In relation to this question of diagnosis, for example, there soon appeared a class of case in which laboratory and clinical opinions were at variance, chiefly cases in which the bacteriological tests had either proved negative or were insufficiently precise to allow of differentiation between the different fevers. In many of these cases the clinicians, either from their failure to find any other cause for the symptoms, or from their feeling that the clinical picture was, in itself, sufficiently clear to warrant the diagnosis, were disinclined to accept the negative results reported from the laboratory. To meet this, it was agreed that such cases should be returned under a separate heading as "enteric group," and this heading, in consequence, figures in all our enteric statistics from the British Expeditionary Force. A similar system was eventually introduced in the other battle areas, but unfortunately too late to affect the major portion of the enteric incidents in their areas.

The diagnosis of "enteric group" was always arrived at after discussion and agreement between the pathologist and the medical officer in charge of the case, and such agreement, I am glad to say, was usually readily reached on account of the intimate associations between ward and laboratory, which is always fostered in army hospitals, and which makes so strongly for good work and for the benefit alike of patient, clinician, and pathologist.

While this concordat between laboratory and ward worked smoothly after a little experience, situations arose at a later stage of the campaign which, if they had been allowed to go by default, or in deference to the side which voiced its views with the greatest emphasis, might—indeed, would—have introduced grave fallacies into all our enteric records. I have in mind chiefly the days when trench fever was a new disease, and only

slowly winning for itself recognition as a disease *sui generis*. Our hospitals at that time were becoming increasingly loaded with cases of continued fever, in the great majority of which the most persistent efforts of the laboratory failed to afford any evidence of their being enteric. In spite of this many medical officers would not admit that these cases could be anything else than enteric, possibly, as they sometimes suggested, modified by inoculation. As time went on, and opinions became clearer and more in concord as to the existence and the symptomatology of trench fever, this difficulty lessened, but for some time it undoubtedly led to an undue swelling of the number of cases returned as "enteric suspects," and also, though to a smaller extent, of those admitted to the final diagnosis, "enteric group." Fortunately, the precautions already existing prevented their entrance into those groups bearing the definite diagnosis of typhoid, paratyphoid "A" or "B."

In the matter of laboratory diagnosis the successful isolation and identification of one of these three germs, at any stage of the illness or post mortem, was of course decisive, and every effort was made by the able and energetic officers in charge of our mobile laboratories, by means of blood and other cultures, to secure such positive information at the most favourable and most useful moment—that of the earliest stage of the attack. When this was successfully effected the information was passed on through headquarters to all the isolation hospitals on the lines of communication, to any one of which the patient might have been sent from the field ambulance or casualty clearing station, according to the exigencies of evacuation needs and of hospital accommodation. When the information was available in time, it was also recorded on the field medical card which accompanied each patient sent down the line.

Time will not permit me to go in any detail into the laboratory methods employed. It must suffice to say that the cultural methods were all of the most modern and approved kinds, and that, within these limits, it was left to the individual pathologist to employ that method with which he was most familiar, and in which he placed most confidence. Agglutination work, however, was on a different footing, and here, on account of its great importance, it was felt that if a method capable of giving accurate and fairly comparable results was forthcoming, it should be employed by all. Such conditions appeared to be fulfilled by Professor Dreyer's method of employing standardized formalin cultures together with a special system of serum dilution, and of the recording of results in terms both of the dilution and of what he has defined as "agglutination units." Although this method has, in some of its details, been subject to criticism, it must be admitted that it constitutes a great advance towards the securing of uniformity, and we found it of great assistance in dealing with the enhanced diagnostic difficulties consequent on the introduction of T.A.B. vaccine and the resultant presence of agglutinins to all three organisms in the blood of the immense majority of the soldiers admitted to our hospitals.

The return sent in for each case of infectious disease was modified to include the principal details of the bacteriological tests carried out in each case, and this form was, on the final disposal of the case, forwarded to the Adviser of Pathology. Uniformity of procedure was aided by each of these forms being studied personally by him, any information found lacking being completed after correspondence with the officer in charge of the hospital concerned. Cases naturally occurred in which the final diagnosis arrived at did not appear to be justified in the light of the recorded details of culture and agglutination; here, too, correspondence with the officers concerned followed, but the final views of those responsible for the case and its diagnosis were invariably accepted.

I now touch on the thorny question of the collection of statistics relating to the incidence of the enteric fevers in the field and to the influence upon them of protective inoculations with either T.V. or T.A.B. The statistical volume of the "Official Medical History of the War" will no doubt set out the figures for the combined returns of all the Expeditionary Forces, scattered over so many theatres of war; but I venture to think that those relating to France may be taken as best worthy of study, for the reason that the British Expeditionary Force, being first in the field and nearest home, was naturally best off, both as regards its equipment and the number of its pathologists, and also in respect of the comparative stability of their working conditions. Further, this force did not at any time suffer from an epidemic so serious as to exclude the possibility of the prolonged bacteriological study of each suspected case. This being so, it was almost always possible to devote to each case the labour necessary for the accurate individual diagnosis of which I have just spoken.

Details as to the character, number, and date of the inoculations in each case had to be recorded on the infectious disease forms, and were forthcoming in the great majority. It might be thought that this should be simple and invariably accurate, but, in spite of the circulation of frequent and stringent orders on the subject, the rules relating to the entry of this information in the soldier's pay book—the only document which is supposed to attach to him everywhere and always—it was inevitable that this, in times of stress, was often lost. Further, over-driven officers and clerks did not always comply with instructions as to the accurate copying of inoculation details from the old pay book when a new one had to be issued to the man. For these reasons, and some others equally unavoidable, it was often necessary to obtain the details of the inoculations verbally from the patient himself, and here, as may well be imagined, we were at the mercy of the man's memory and accuracy, and although this could usually be relied on, there were many proven instances of incorrect information being obtained in this way. Here we could do no more than record in each case on the return whether the facts of inoculation were obtained from documentary information or from the man's own statement.

Nominal rolls of inoculation of units might have obviated such doubts, but, as the armies grew to millions, this would have necessitated the provision of a large staff and a toil of searching and correspondence out of all proportion to the value of the quest.

Another very important point, which must be borne in mind in connexion with all war statistics, is the fact that the strength, the foundation of all returns relative to incidence, is an inconstant figure. During the first three years of the war the strength of the British Expeditionary Force was a constantly increasing strength, the numbers growing from a few hundred thousand to two million men, but, in addition to this, the individuals comprising each unit, whether a platoon or an army corps, were continually changing. The heavy toll of battle casualties and of invaliding from other sources, as well as the transfer of individuals or whole units out of or into the British Expeditionary Force, make any attempt to analyse the inoculation and enteric history of an individual unit a vain task. Further, these fluctuations and changes in the individuals comprising the unit introduce, on the widest possible scale, the grave fallacy of lack of uniformity in the periods to which the individuals had been exposed to the dangers of infection. Even if a correct statistical formula could be furnished, its interpretation would, I think, still present manifold difficulties. The display of the collected figures in the form of the annual incidence per 1,000 or per 100,000 of mean strength—the form which the official returns will probably take—gives an impression of accuracy; but, in view of the points I have mentioned, I am inclined to think that a somewhat better appreciation of the situation may be obtained from a study of the comparative tables showing the total number of cases in each group which had occurred in the British Expeditionary Force up to certain dates, and that these should be read in the light of the average percentage of inoculation existing in the Force at that time.

The "daily state" of all infectious diseases was wired every day to the Director of Medical Services, lines of communication, and those relating to enteric and certain other diseases I entered in my own note-books during the three and a half years I was adviser in pathology to the Force. It is from these figures, verified and checked by a close analysis of the individual case-sheets, that the tables which I show you were compiled:—

TABLE I.—TOTAL CASES AND CASE-MORTALITY FROM TYPHOID FEVER, BRITISH EXPEDITIONARY FORCE—AUGUST, 1914, TO JANUARY 29, 1915.

	Cases	Deaths	Case-mortality
Not inoculated within 2 years	305	34	11·1
Inoculated 1 dose T.V. within 1 year	83	1	1·2
Inoculated 2 doses T.V. within 3 years	33	0	0 0
Totals	421	35	Average 8·3

TABLE II.—TOTAL CASES AND CASE-MORTALITY FROM TYPHOID AND PARATYPHOID, BRITISH EXPEDITIONARY FORCE, AUGUST, 1914, TO APRIL 28, 1915.

	Cases	Deaths		Case-mortality
Typhoid—				
Not inoculated within 2 years	481	100		20·79
Inoculated 1 dose T.V. within 1 year	157	10		6·36
Inoculated 2 doses T.V. within 2 years	142	10		7·04
Totals	780	120	Average	15·38
Paratyphoid—	281	10		3·55

TABLE III.—TOTAL CASES AND CASE-MORTALITY FROM TYPHOID AND PARATYPHOID, BRITISH EXPEDITIONARY FORCE—AUGUST, 1914, to AUGUST 9, 1915.

	Cases	Deaths		Case-mortality
Typhoid—				
Not inoculated T.V. within 2 years	545	111		20·36
Inoculated 1 dose T.V. within 1 year	190	13		6·84
Inoculated 2 doses T.V. within 2 years	236	19		8·05
Totals	971	143	Average	14·72
Paratyphoid—				
Not inoculated T.V. within 2 years	113	4		3·53
Inoculated 1 dose T.V. within 1 year	183	3		1·63
Inoculated 2 doses T.V. within 2 years	423	8		1·89
Totals	719	15	Average	2·08
Typhoid Group—				
Not inoculated T.V. within 2 years	24	0		0
Inoculated 1 dose T.V. within 1 year	27	0		0
Inoculated 2 doses T.V. within 2 years	43	0		0
Totals	94	0		0

TABLE IV.—INCIDENCE OF TYPHOID IN BRITISH EXPEDITIONARY FORCE DURING 1915—APPROXIMATE AVERAGE STRENGTH OF THE BRITISH EXPEDITIONARY FORCE, 643,000—AVERAGE PERCENTAGE OF INOCULATION WITH T.V., 94 PER CENT (INCLUDING MEN WITH ONE DOSE ONLY).

	Average strength	Total cases	Incidence per 1,000	Total deaths		Death-rate per 1,000
Inoculated T.V., 1 or 2 doses	604,420	570	0·94	34		0·056
Not inoculated T.V.	38,580	295	7·64	89		2·306
Totals	643,000	865	—	123	Average	0·191

Ratio of incidence in inoculated to incidence in non-inoculated .. .. .. 1 to 8·1
,, death-rate ,, death-rate ,, ... 1 ,, 41·1

I must myself observe the caution which I earlier recommended to you in comparing these figures with similar ones relating to the same facts among either our Allies or our enemies, or even within the limits of our own scattered armies. Differences in such factors as the attention which it was possible to give to individual diagnosis, the nature of the vaccine employed, and other variables, make such comparisons treacherous; but it is not unfair, I think, to contrast our own fortunate escape from any grave epidemic in France with the experience of the French who had in their armies on the Western Front in the first sixteen months of the war, 95,809 cases and 11,690 deaths from this group of diseases. We ourselves, within approximately the same period, had but 1,205 cases of typhoid with

152 deaths, 1,331 cases of paratyphoid with 18 deaths, and 153 cases returned as "enteric group" with no deaths—a total of 2,689 cases and 170 deaths. Our colleagues of the French medical service have themselves no shadow of doubt that they owed their subsequent relative immunity from these diseases, which eventually became as striking as our own, to the subsequent thorough inoculation of their forces with vaccines prepared either by the method of M. Vincent or that of the late Professor Chantemesse.

TABLE V.—TOTAL CASES AND CASE-MORTALITY OF TYPHOID AND PARATYPHOID—BRITISH EXPEDITIONARY FORCE DURING 1916.

	Cases	Deaths		Case-mortality
Typhoid—				
Inoculated T.V. or T.A.B.	739	14		1·89
Not inoculated T.V. or T.A.B.	42	3		7·14
Totals	781	17	Average	2·17
Paratyphoid—				
"A." Inoculated T.A.B.	338	4		1·18
Not inoculated T.A.B.	173	3		1·73
Totals	511	7	Average	1·36
"B." Inoculated T.A.B.	534	3		0·56
Not inoculated T.A.B.	560	7		1·25
Totals	1,094	10	Average	0·91
Totals—Para. "A" and "B"	1,605	17	Average	1·05
Enteric Group—				
Inoculated T.V. or T.A.B.	368	0		0
Not inoculated T.V. or T.A.B.	6	0		0
Totals	374	0		0
Totals—Typhoid, paratyphoid and enteric group	2,760	34		1·23

Before passing to some other aspects of the subject, it would be most unjust if one omitted to refer to the important part played by our colleagues at home in aiding us to keep these diseases at arm's length, and to prevent their spreading in epidemic form among the troops. This work was, in the main, directed into two channels, first, to endeavour to send over to us each fresh unit or draft with as high a percentage of inoculation as possible, and second, to ensure that convalescents of enteric were not permitted to join or rejoin an Expeditionary Force until it was clear that they were no longer excreting the infecting bacilli.

As regards the first of these functions, the high state of inoculation which it was the rule for us to find among drafts and reinforcements bore ample evidence to the fruitful results of their labours; indeed, it was no rare occurrence to hear of the arrival of a new battalion with every officer and man inoculated. Smaller units and mixed detachments presented greater difficulties, but in these cases, by arrangement with the War Office, we were notified at once of their departure and destination, and

were then able, on our side of the Channel, to follow them up and endeavour to rectify matters.

TABLE VI.—TOTAL CASES AND CASE-MORTALITY OF TYPHOID AND PARATYPHOID, BRITISH EXPEDITIONARY FORCE DURING 1917.

	Cases	Deaths		Case-mortality
Typhoid—				
Inoculated T.V. or T.A.B.	217	11	..	5·06
Not inoculated T.V. or T.A.B.	42	4	..	9·52
Totals	259	15	Average	5.79
Paratyphoid—				
"A" Inoculated T.A.B.	149	1	..	0·67
Not inoculated T.A.B.	41	1	..	2·43
Totals	190	2	Average	1·05
"B" Inoculated T.A.B.	366	2	..	0·54
Not inoculated T.A.B.	151	2	..	1·32
Totals	517	4	Average	0·77
Totals—Para "A" and "B" ..	707	6	Average	0·84
Enteric Group—				
Inoculated T.V. or T.A.B.	291	2	..	0·68
Not inoculated T.V. or T.A.B.	18	1	..	0·55
Totals	309	3	Average	0·97
Totals—Typhoid, paratyphoid, and enteric group	1,275	34	..	2·66

The second branch of the work at home was to continue and extend the search for carriers. This called for very careful organization as well as for skilled and laborious laboratory work, but it was not long before this side also attained a high degree of efficiency. The carrier problem in general is a sufficiently difficult one to deal with in times of peace, but in war, although our powers are greater, the difficulties are enormously multiplied, while the grave potentialities consequent upon the presence of undetected germ-carriers among troops in crowded and often insanitary surroundings are obvious. In the British Expeditionary Force we made it a rule that every diagnosed case of typhoid, paratyphoid or enteric group infection should, when fit for transport, be invalided home, neither the accommodation nor the conditions being altogether suitable in France for the necessary prolonged period of convalescence. Consequently, it fell to the medical authorities at home to carry forward and complete the bacteriological tests requisite to establish, with reasonable certainty, the eventual freedom of the convalescents from the carrier condition.

To this end, centralization of the returned convalescents was obviously the best means of securing both uniformity of practice and economy of labour. This was arranged for by the creation of typhoid convalescent depots, and eventually a single depot, situated at Addington Park. In these depots the case was carefully followed up, and the rule observed that no patient should be allowed, under any circumstances, to return to an Expeditionary Force until at least three consecutive bacteriological examinations of the excreta—conducted at intervals of seven days—should have proved negative. Cases in which the chronicity of the carrier

condition had already been established, either over-seas or at home, were naturally subjected to a still more rigorous standard of cure, in view of the well-known intermittency which such chronic carriers may at times present. These precautions worked excellently, and it was very rare for us to encounter a healthy carrier in France, at any rate among our own soldiers. Cases, however, did occur, usually detected as the result of the rigorous investigations undertaken in connexion with some small explosion of fever of uncertain nature in a unit. In a number of these a typhoid or a paratyphoid carrier was run to earth, and usually found either to be a man who had had an attack of one of these fevers before the war or who, during the war, usually through some error of diagnosis, had escaped the meshes of the net spread for his capture. No system of this kind can ever be perfect when one has to deal with men by the hundred thousand, who lived in the closest association not only with our own Colonial troops and native contingents from many parts of the Empire, but also with the forces of our Allies and, the greatest danger of all, with masses of the civil population and refugees, crowded at times in the billeting areas behind the lines.

That our enteric incidence in the British Expeditionary Force was maintained at a level so extremely low I myself am inclined to attribute in no small measure to the efficiency with which this organized campaign against the carrier was conducted. My views on the protective value of inoculation are, I fancy, sufficiently well known to you, and I have already paid a tribute to the efficiency of field sanitation; but, whatever opinion be held as to the relative influence of these, there can, I think, be no divergence as to the value of this third line of defence—a systematized search for carriers and their strict exclusion from the fighting line till cured. It is a great labour, but one which is indeed worth while, and those patient pathologists who spent long days of toil, and often nights as well, in this laborious and often monotonous work may well feel that they have served their country in her need.

So far, I have spoken of the enteric fevers in general terms, without distinction between the various forms. I may now say a little on the incidence and other points in connexion with the paratyphoid fevers. In this case we had but little to guide us from the experiences of former wars. In the South African War, although paratyphoid had been recognized for some little time, no systematic search for it was made on any scale at the time of the heavy incidence of enteric, and cases were but rarely diagnosed. From subsequent experience in South Africa we have learnt that paratyphoid fevers are not uncommon, but we cannot be certain whether this may not have been a legacy from the war. It is, however, legitimate to attribute a certain responsibility for the comparatively disappointing results of typhoid inoculation in that campaign to the fact that the inoculated men, in an unknown proportion of cases, were attacked by paratyphoid fevers, against which they had not been protected.

We had, however, nothing definite or precise to guide us in our anticipations in connexion with the incidence of paratyphoid fever in France. The first cases appeared very soon after the first typhoids, but continued rare and isolated until 1915 was well advanced, and the great majority of the cases admitted to our hospitals from the troops fighting behind the Aisne were true typhoids. This, although proved by the laboratory tests, received abundant confirmation, had it been needed, from the clinical picture and the case-mortality. Many of these earliest cases which I saw at No. 4 General Hospital at Versailles in October, 1914, recalled vividly to my memory, by reason of their severity, the evil days of typhoid in India in the early 'nineties. Such cases were almost without exception in uninoculated men.

Later, on the transference of our lines to Flanders, paratyphoids became more frequent among our men; and there is little doubt that this was in the main due to the prevalence of these fevers, in addition to genuine typhoid, among the Flemish and French refugees in the villages around Ypres and Armentières. These unfortunate people were subjected to a severe epidemic, which did not disappear until it was taken in hand by house-to-house visitation, and by arrangements being made for the segregation and hospital treatment of the cases at Poperinghe, Malassise, and Montreuil. Protective inoculations were also widely carried out, being readily accepted by the people. It was, by the way, a curious experience in modern war to visit a military hospital and find wards full of women and babies suffering from or convalescing from enteric fever. I should also like to pay a tribute to the excellent work done in connexion with this civilian outbreak by an ambulance unit worked by the Society of Friends, who carried out their arduous and often dangerous work with the greatest efficiency and gallantry.

By degrees the number of cases of paratyphoid under treatment in our hospitals came to exceed those of typhoid, the lines crossing one another in the month of June, 1915, and from that time till the end of 1918 paratyphoids retained this preponderance. Both forms were represented, but paratyphoid "A" was always rarer than paratyphoid "B," our experience in this being the reverse of that of our French colleagues.

In view of the general recognition of the protective value of typhoid vaccine, it was natural that similar hopes should be held on the possibilities of protecting against paratyphoid infections, either by a separate vaccine or by one in which paratyphoid bacteria were mixed with typhoid. Those who held such hopes became increasingly urgent in their representations as the number of cases increased among the Belgian and French refugees, and, though to a small extent, among our own men. Personally, I was not altogether convinced as to its possible value, for reasons with which I need not trouble you in detail, but may summarize briefly as follows:—

(1) No paratyphoid vaccine had so far been employed upon a scale and under conditions sufficient to establish the degree, if any, of its protective power.

(2) It was obvious that a considerable dose of both paratyphoid organisms would be necessary to secure any effective degree of immunity; it was therefore possible that the mass effect of the immunizing doses of the three organisms—presuming a mixed vaccine was aimed at—might cause local and general reactions so severe as to be undesirable in themselves and deterrent in their effect upon voluntary inoculation.

(3) There was no certitude that the addition of the paratyphoid fractions would not exercise a deleterious effect upon the immunizing power of the more essential typhoid fraction.

Many discussions were held on these and other points, and eventually, early in the autumn of 1915, I returned to London to take part in a discussion of the whole question by experts called together by the War Office for its settlement. After a full discussion it was agreed that a mixed vaccine—T.A.B.—should be introduced in replacement of the simple typhoid vaccine—T.V.—hitherto in use, provided that certain experimental tests and trials, agreed upon as likely to settle some of the doubtful points of which I have just spoken, should be carried out, and should yield satisfactory results.

These experiments were carried through successfully under the direction of Colonel D. Harvey, who held the very responsible post of Director of the Vaccine Department of the Royal Army Medical College throughout the war, and they resulted in establishing the points that a sufficiently large dose of para. "A" and para. "B" vaccine could be added to the customary dose of typhoid vaccine without causing reactions appreciably more severe than those of T.V. by itself, and without interfering with the satisfactory development of typhoid immune substances in the blood of inoculated animals and inoculated men. As regards the development of immune substances against the two paratyphoid organisms, this was also regarded as fairly satisfactory, though here we had no effective standards to work by, while the actual degree of protection against paratyphoid infection had obviously to be left for future decision from statistical evidence.

The actual formula of the mixed vaccine determined upon, and used for the rest of the war, was as follows:—

	First dose	Second dose
B. typhosus	500 millions	1,000 millions
B. paratyphosus A ..	375 ,,	750 ,,
B. paratyphosus B ..	375 ,,	750 ,,

In order to minimize as far as possible the statistical difficulties which were bound to follow on a change of vaccine, we decided to endeavour to make this change, as far as possible, upon the same date throughout the whole of our armies at home and abroad. To this end, when a sufficient supply of the new vaccine was ready, it was distributed to all theatres of war and Commands at home some time beforehand, and precise instructions were circulated as to the date on which T.V. inoculations were to cease and T.A.B. to commence. On the whole this worked well, and we

were later in a position to assume, in the absence of written evidence, that any man inoculated against enteric fever on a date later than the beginning of February, 1916, had been done with T.A.B. and not with T.V.

If need be, and if of sufficient importance, the question could always be put to the proof in the laboratory, where the blood of a number of men of the unit in question could be examined, and the presence or absence of paratyphoid agglutinins determined.

As to the results of the mixed vaccine, while I think it is clear that it has been successful in securing to the Army a considerable measure of protection against paratyphoid infections, in addition to that against typhoid itself, I think the protection against the former has proved of a lower grade than that against the latter. This is, however, a point upon which it is extremely difficult, if not impossible, to secure convincing statistical evidence, in view of the multiplication of embarrassing statistical factors and possible fallacies. For myself, the matter is still further complicated by a conclusion which appeared to me to arise from a study of our enteric incidence in France during the period which preceded the introduction of T.A.B. vaccine. This was that inoculation with T.V. alone had conferred an appreciable degree of protection against paratyphoid in addition to the marked immunity it gave against typhoid. This, to me, was entirely unexpected since, in our long series of experiments in connexion with typhoid vaccine, we had never been able to demonstrate the presence of paratyphoid immune substances in the blood of men or animals inoculated with typhoid vaccine. The group affinities of the three are so close that evidence of some degree of co-immunity might, on theoretical grounds, have been anticipated, but this was entirely lacking. The figures from my records—for instance, the comparative case-mortalities shown in Table III—are difficult to interpret on any other ground, and I find it difficult to avoid the conclusion that they suggest—one cannot, I think, put it more strongly—that the typhoid inoculations have had a favourable influence on both the incidence and the severity of attack of paratyphoid. The figures, as far as they go, appear to show a more pronounced effect of the vaccine upon the severity of the attack, as judged by the case-mortality, and this appears to me to strengthen considerably the assumption that the typhoid vaccine had given some immunity against paratyphoid. I cannot spare more time to this matter, but would remind you, in concluding it, that the negative results of laboratory tests of such a co-immunity are concerned chiefly with the failure to demonstrate the presence of specific paratyphoid agglutinins in men or animals inoculated with typhoid vaccine. The presence of such agglutinins is generally held, in bacterial diseases of this class, to be a rough measure of the degree of immunity conferred, but there are many and puzzling exceptions to this, and we are still too uncertain, both as to the true nature of agglutinins and the method of their production, to build any very heavy fabric of hypothesis upon a foundation so insecure.

If, then, it be the case that the typhoid fraction of the mixed vaccine is capable of conferring a degree of non-specific immunity, effective or partially effective against paratyphoid infections, it will be clear that the difficulties of determining the extent to which T.A.B. vaccine was better than T.V. are increased. On the whole, however, the point is of greater interest from the scientific than from the practical standpoint, since it is undoubted that the addition of the paratyphoid fractions has had no deteriorating effect upon the degree of protection given against the graver disease, while the persistent low incidence of the paratyphoid fevers in a force highly inoculated with T.A.B. may be accepted with a confident feeling that a considerable part at least of this immunity was due to the inoculations.

Unfortunately, little light is thrown upon the matter by a study of the paratyphoid incidence in the British Expeditionary Force subsequently to the introduction of T.A.B. for the reason that a remarkable drop occurred in this incidence during the latter half of the year 1915—that is, prior to the adoption of the mixed vaccine—and the most that would appear possible to gather from the returns is that the already low incidence of these fevers was kept low after the change of vaccine.

The matter will no doubt be capable of closer analysis when the figures from the other Expeditionary Forces are available for study, and in some respects it is to be anticipated that the experiences in some of the Eastern theatres of war will afford better material in view of the heavier incidence of the enteric fevers in these forces. Unfortunately, however, the important factor of lack of precision in diagnosis, on which I laid stress earlier, played a large part in these forces at the time at which these fevers were prevalent in epidemic form.

Allusion must next be made to another possible statistical fallacy, the extent of which can only be estimated with a considerable degree of vagueness. The generally mild nature of an attack of enteric fever in an inoculated man is now well recognized, and, particularly in the case of typhoid fever, has greatly added to the difficulties attaching to a diagnosis made solely on the clinical evidence. Although, in my experience, inoculation does not modify a subsequent attack of paratyphoid to anything like the same extent, yet here, too, there are encountered many cases so mild, or so atypical, that only prolonged and careful laboratory work can establish their true nature. It results from this, that unless all cases of continued fever, of whatever nature, are subjected to an examination as thorough and exhaustive as in the case of a definite "enteric suspect," an unknown number of mild infections with typhoid and paratyphoid fever may, indeed must, escape detection and pass to their convalescence under the official label of some other disease—in many cases, no doubt, the perfectly correct label of some affection which ran a course concurrent with a masked enteric attack.

This was so clear and so inevitable that it gave us at various times grave

preoccupations, chiefly in relation to the possible existence of a dangerous loop-hole in our organization for the detection and segregation of carriers. A number of attempts were made, some of them on a very considerable scale, to ascertain the proportion of such undetected or erroneously diagnosed cases in a military community at a given moment. These were carried out principally in large hospital establishments, either at home or in an Expeditionary Force, but others were done in France on healthy troops while in rest billets behind the lines. The estimates arrived at have varied very widely, as was perhaps to be expected from the widely divergent opportunities for past or recent infection to which the various bodies of men had been exposed. For instance, men from a unit which had recently served in an Eastern theatre, where they had been exposed to epidemic dangers greater than those whose service had been limited to the Western front, always showed a higher proportion of undetected cases. Owing to these variations in the conditions of service, and many others to which I have no time to allude, it is not possible to do more than give a very general statement as to the average percentage of such cases among the men admitted to our hospitals, either as sick or wounded. I do not think myself that this figure can have exceeded one per cent, though individual results have sometimes shown a higher proportion. However this may be, the results of all these inquiries were most reassuring from the practical viewpoint from which they were instituted, namely, the quest of the undetected carrier. Such were found with extreme rarity, and our fears in relation to this potentially grave danger were set at rest.

Undetected carriers of typhoid or paratyphoid being then so very rare, proof of a recent unrecorded attack had to rest almost entirely on the results of a series of endpoint estimations of the agglutination-titre of the serum to typhoid, paratyphoid "A" and paratyphoid "B," respectively. Here acknowledgment must again be made to the method of enhanced precision introduced by Professor Dreyer, and to the facilities for its employment enjoyed throughout the armies, thanks to the organization and generosity of the Medical Research Council. There could be no question of the very high value of the method in assisting at the drawing of correct conclusions as to the true nature of these difficult cases. In many it made a definite diagnosis simple, in most, highly probable, and only in a minority did it leave an element of doubt in the mind of the critic, if not in that of the observer himself. When the greatest force has been admitted to the various criticisms which have been made upon parts of the Dreyer technique, it still remains beyond question that it is a great advance upon older methods of agglutination measurement, and I may further bear testimony to the fact that most, if not all of our pathologists in France came to think well of it in direct proportion as their experience of it increased.

A number of interesting observations relating to the diagnosis, course, and treatment of the enteric fevers have been made during the war, and

many of these have already appeared in the medical press. Most of them, however, are supposed to lie outside the interests of the pathologists, and if I venture in conclusion to allude to one or two of them it is because they are, in greater or lesser degree, associated with the work of the laboratory.

(1) *Double Infections.*—Cases of this nature are always of great interest, and are almost always very puzzling until the bacteriological proof of their true nature has been established. A definite diagnosis founded solely upon the clinical evidence is rarely possible and usually, at the best, conjectural. More definite evidence may be forthcoming from an expert consideration of the curves of the three agglutination indices; but here too, it is exceptional for these curves to follow a course so definite that a diagnosis may be accepted without reservation. There have, however, been instances, isolated, though in their total fairly numerous, in which the diagnosis has been established beyond doubt by the isolation of both germs during the course of the illness. As far as I can recall every possible combination has been encountered, namely, a mixed infection by the two paratyphoids, and typhoid associated either with paratyphoid "A" or paratyphoid "B." It was rare to isolate the two germs responsible for the double infection from the same source; more frequently one was obtained from the blood, and the other, after perhaps a considerable interval, from the fæces or the urine. The occurrence of such double infections in which the isolation and identification of the two germs had made the diagnosis certain was of great value in assisting the interpretation of the agglutination curves of suspected double infections, and, as experience accumulated in this direction the impression was formed that infections of this nature were less uncommon than had been supposed. The task of laboratory confirmation, however, remained a very difficult one owing to the fact that, in the case of men who have had T.A.B. vaccine, the agglutination-titres of their serums are often raised not only against the bacillus which is the cause of their attack but against the others as well, though usually to a less marked degree. Still, making every allowance for this, cases were not uncommon in which the curves built up from the recorded estimation of the three agglutination indices were very suggestive that more than one of the group had been at work.

(2) *Isolation of the Bacilli.*—Inoculation appears to have diminished the chances of successful isolation of the bacilli from the blood at the time when this would be of the greatest service from the diagnostic point of view, namely, the early stage of the fever; but every effort was made to secure this valuable information while it might be available, and a large part of the work of our mobile laboratories behind the lines consisted in making blood cultures from suspicious cases of continued fever in the field ambulances or casualty clearing stations. By the time a case had been transferred down the line and had reached one of our isolation hospitals the chances of success from this source had greatly diminished, and reliance had chiefly to be placed upon the more laborious methods

of isolation from fæces or urine. These efforts, if repeated at intervals, were often successful in the end; but, as has been said, the final laboratory diagnosis had in the majority of cases in inoculated men to rest upon the interpretation of the agglutination curves.

This influence of inoculation in the inhibition or diminution of the degree of bacteriæmia was illustrated in a striking manner on several occasions within my experience. On studying a series of enteric case sheets one would be struck by the unaccustomed frequency with which reports of positive blood cultures were found in those sent from a certain station during a certain period. On making further inquiries, usually upon the spot, I found that the cases in question were from a small explosion of enteric in some unit, usually a non-combatant one, the individuals of which had been sent overseas very inadequately protected by inoculation, and the cases in question were, I think almost without exception, in men who had not been inoculated. It may be added, that here, once again, one encountered the severe and toxic forms which it was the exception to see in inoculated men. If further evidence on this point were needed, we need seek no further than the figures of the case-mortality of typhoid among the inoculated and non-inoculated respectively. In all tables which I show you—and they were not selected from this point of view—the heavier mortality among the non-inoculated will be evident. I may add that a similar reduction in the case-mortality of paratyphoid, following on the introduction of T.A.B. vaccine, is not evident, and this appears to be a further indication that this vaccine has had little influence in modifying the severity of an attack of paratyphoid in men whom it has incompletely protected against infection.

(3) *Vaccine Treatment of Enteric Fever.*—Pre-war experience had led one to think that the early employment of therapeutic vaccines might be of considerable value in the treatment of the enteric fevers. Trials of the method in many countries and with very wide variations both in the nature of the vaccine employed and the system of dosage had, on the whole, given very promising results, reflected more in a substantial reduction of the case-mortality than in a diminution of the average duration of the pyrexia. Our ordinary prophylactic typhoid vaccine, prepared from the now historic "Rawlings," had been used for treatment in several series of cases and had given very encouraging results, as, indeed, had most of the other vaccines used for the same purpose. It was, however, generally felt that fuller information was needed on such points as the class of case most suitable for its employment, the size and frequency of the doses, and the indications for modifying these latter factors during the course of the attack to the greatest advantage of the patient.

It was anticipated that opportunities for multiplying our experience in this direction would not be lacking during the war, and, accordingly, a leaflet of instruction, containing suggestions as to dosage, etc., was circulated to all hospitals, while an appropriate dilution of the prophylactic

vaccine was prepared and issued in separate phials as "vaccine for treatment." The scope for the employment of such a vaccine, was, however, in actual fact, found to be greatly circumscribed, and that for a most gratifying reason. The great majority of the cases of typhoid actually encountered were so modified in their severity, presumably on account of their previous prophylactic inoculation, that little additional benefit was to be expected from the further employment of vaccine as a therapeutic agent. One did not, under these circumstances, feel justified in pressing the method upon the officers responsible for the treatment in the case of patients who were obviously doing well and in a fair way to convalescence. In the larger isolation hospitals, however, better opportunities presented themselves, especially in the earlier months of the war, when cases among non-inoculated men were seen in numbers sufficient to permit of control observations among those of a sufficient degree of severity.

Systematic trials of the method were accordingly conducted both at No. 14 Stationary Hospital at Wimereux and at No. 10 General Hospital, Rouen. I was greatly interested personally in this subject, and paid many visits to these hospitals while the method was being tested, examining the records of the cases and the men themselves, and having many interesting discussions on the subject with the officers in clinical charge, with the pathologists and with the consulting physicians—in particular with Sir Bertram, now Lord Dawson, who took a great interest in the trials. No pains were spared to make these as thorough and as clear of interpretation as was possible, and the series of treated cases and of controls dealt with at Wimereux, in particular, was sufficiently large to form a fair basis for judgment. In general there resulted an agreement among the responsible clinicians that the favourable results occasionally obtained were too inconstant, and that the method as a whole was too little under control to justify dependence being placed upon it as a routine measure. That better results might have been obtained by the employment of autogenous vaccines is probable, but there are very obvious pratical limitations to this method, while that of treatment by the use of living cultures, as had been tried by several observers with results which they regarded as good is one which one would hesitate to advocate for general use under war conditions. In view of the results of these inquiries in our own hospitals, details of which have been put on record in the medical press, the method of vaccine treatment was practically abandoned in France during the later years of the war. Personally I do not feel that the matter can be considered as finally closed, and I believe that further work, directed, perhaps, towards the determination of those elements of a bacterial vaccine which alone are essential for immunization, may yet lead to the production of a vaccine and of a system for its employment capable of great therapeutic benefit in severe cases of typhoid and paratyphoid fever. My own examination and analysis of the cases which I saw under treatment in the various hospitals led me to a conclusion rather more favourable than that arrived at by my colleagues,

and I find that my notes made at the time record that the general condition of the patients appeared to be greatly improved by the treatment, although the pyrexia was not curtailed. The incidence of severe complications was also less in those who had been treated than in those who had not. These favourable results were noted to be most frequently observed in cases in men who had not been inoculated prophylactically.

In conclusion, I would ask your indulgence for what I feel to have been a sadly discursive and unsystematic lecture, particularly unworthy in this respect of the memory of the distinguished physician in whose honour it has been founded. My only excuse is that I had thought, perhaps wrongly, that it would be of more general interest if I tried to touch, albeit lightly, on several different sides of this large subject, rather than to treat of a single portion in a more detailed and documented fashion.

AN ASSISTANT DIRECTOR OF MEDICAL SERVICES OF AN ARMY IN WAR TIME.

By Lieutenant-Colonel W. F. TYNDALE, C.M.G.
Royal Army Medical Corps.

(*Continued from p.* 416, *vol. xxxvi.*)

A memo, "Instructions to Entraining Medical Officers" should be drawn up in quiet periods and issued to acting entraining officers, keeping spare copies for future use. I again regret that I have no copy of this available, and my memory fails me as to details. Officers in command of casualty clearing stations should be asked for a return of their entraining medical officer, and the names of those who could act, if necessary, as assistants. A list of these should be kept in the office for reference and future use, and the Assistant Director of Medical Services should make himself acquainted with the comparative merits of the officers. If necessary, before a "strafe" he can move these officers about, but should do so sufficiently soon to allow them to "settle in." The most expert should go where the work is heaviest. Where the casualty clearing stations are grouped, *one* officer must be in supreme control, and, in order to get a backing, he should be attached to the casualty clearing station of the senior officer of the group. Officers in command of casualty clearing stations may be inclined to interfere on behalf of their own hospital, and he may be torn by contradictory orders. Sometimes the officer in command of a casualty clearing station arranges for the entraining, etc., and the entraining medical officer carries out the actual supervision. If this works well in a group without friction, it should not be interfered with. It is most important both in peaceful periods and during operations for either the Director of Medical Services or the Assistant Director of Medical Services to be present occasionally (unexpectedly) at the loading of an ambulance train. It may be an eye-opener either of the efficiency of the arrangements or the reverse. No less important are effective arrangements for clearing loaded motor ambulances on arrival at a casualty clearing station (single or grouped). If badly managed, a line of waiting ambulances half a mile long or more may be seen. Corps begin to complain that their cars go off to casualty clearing stations and are not seen again for the day. Complaints may even arrive from the General Staff who have noticed the block. Corps evacuations and army evacuations are greatly delayed, and much additional suffering evolves on the wounded. In respect of this I might mention here that motor ambulance convoy officers and non-commissioned officers on motor cycles should be told off to supervise the running of cars along the various main routes.

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A memo, "Instructions to Entraining Medical Officers" should be drawn up in quiet periods and issued to acting entraining officers, keeping spare copies for future use. I again regret that I have no copy of this available, and my memory fails me as to details. Officers in command of casualty clearing stations should be asked for a return of their entraining medical officer, and the names of those who could act, if necessary, as assistants. A list of these should be kept in the office for reference and future use, and the Assistant Director of Medical Services should make himself acquainted with the comparative merits of the officers. If necessary, before a "strafe" he can move these officers about, but should do so sufficiently soon to allow them to "settle in." The most expert should go where the work is heaviest. Where the casualty clearing stations are grouped, *one* officer must be in supreme control, and, in order to get a backing, he should be attached to the casualty clearing station of the senior officer of the group. Officers in command of casualty clearing stations may be inclined to interfere on behalf of their own hospital, and he may be torn by contradictory orders. Sometimes the officer in command of a casualty clearing station arranges for the entraining, etc., and the entraining medical officer carries out the actual supervision. If this works well in a group without friction, it should not be interfered with. It is most important both in peaceful periods and during operations for either the Director of Medical Services or the Assistant Director of Medical Services to be present occasionally (unexpectedly) at the loading of an ambulance train. It may be an eye-opener either of the efficiency of the arrangements or the reverse. No less important are effective arrangements for clearing loaded motor ambulances on arrival at a casualty clearing station (single or grouped). If badly managed, a line of waiting ambulances half a mile long or more may be seen. Corps begin to complain that their cars go off to casualty clearing stations and are not seen again for the day. Complaints may even arrive from the General Staff who have noticed the block. Corps evacuations and army evacuations are greatly delayed, and much additional suffering evolves on the wounded. In respect of this I might mention here that motor ambulance convoy officers and non-commissioned officers on motor cycles should be told off to supervise the running of cars along the various main routes.

Cases occur in which a car having unloaded and being due to return to its corps turns off instead into a side road out of sight, and the driver either eats or sleeps as long as he feels inclined. The officer in charge of all army cars and attached cars, buses, etc., should be seen by the Assistant Director of Medical Services in his office before a "strafe," and amongst other things the Assistant Director of Medical Services should instruct him if he has not already made such arrangements to issue definite orders as far as possible for drivers as to feeding rations to be carried, meals to be served at headquarters, periodical assignment of cars to the reserve to rest the drivers, and so on. I remember one instance, in particular, which emphasizes the necessity both of these definite orders and the need of watchful motor ambulance convoy officers. In the advance at one time, owing to breakage of the railway line by delayed railway mines, it was necessary to evacuate the front line casualty clearing station by car, bus, and lorry fifteen to twenty miles to casualty clearing stations in the rear; for this purpose with difficulty we had obtained the use of about eighty cars, forty buses, and a few lorries. If the cars could make daily two to three journeys and the buses two, we could keep the front casualty clearing station fairly clear; but we found that many of them going to a certain group of casualty clearing stations were not making anything like this number of journeys. On inquiry it was found that one of the officers in command, in the kindness of his heart, was allowing each driver, on unloading, to go off and have a meal, and that there was practically no check on their time of leaving the casualty clearing station. It was discovered that meals were arranged at the other end by the motor ambulance convoy officer, so that this additional meal was unnecessary; but one could hardly expect a tired cold driver to forgo the extra meal and rest of his own accord.

The majority of entraining officers were found until late in the war from among the dental officers and chaplains. Dental work in a "strafe" was at a standstill usually and hence these officers were usefully employed. Very good some of them were. The same remark applies to some of the chaplains. The duty needed much energy, organizing power, and skill in handling men who often were tired from overwork and want of sleep. It was rarely possible to get as many bearers as one would like and therefore they were generally heavily worked day and night. Objections were made later on in the war to the employment of chaplains as entraining medical officers and deservedly so. Although it was a loss to the medical arrangements one could not reasonably complain. At the busy groups of casualty clearing stations an assistant entraining medical officer must be appointed as well, possibly two assistant surgeons. Rest must be obtained and loading and unloading of cars or trains often continued day and night. It is also essential that the entraining medical officer should have a clerk who should be given every facility for telephoning and who should always be within reach of the 'phone in case he is called up by the Director of

Medical Services' office. The Assistant Director of Medical Services should see that all this is arranged carefully beforehand if he wishes to avoid trouble. The Assistant Director of Medical Services should always have in full view in his office a blackboard showing the exact state of each casualty clearing station as regards available accommodation, numbers in hospital, numbers for evacuation, etc. These should be kept up to time as far as the facilities of communication (often bad) allow. On another board or on a paper on the table should be a list of the ambulance trains in the area, giving their official number, and their latest situation on the railway line as obtained from the railway authorities. The accommodation in these as regards sitting and lying is shown on the official list issued. These trains will be directed to the groups where most urgently required, or where pressure of railway traffic allows. Judgment is required in this allotment as for example, it may be wiser to send a train to a less crowded group of casualty clearing stations because the line to it is less heavily engaged.

Although I am diverging somewhat from the discussion of entraining officers, it is not inopportune here to give the Assistant Director of Medical Services' duties before and during an operation as regards the supply and working of ambulance trains.

The number of ambulance trains that it is possible to work in during a "strafe" depends on many factors and not the least on the Deputy Assistant Director of Regimental Transport of the army and officer of the same department who runs the ambulance trains. Like all the other branches these officers vary in ability, and given the ability whether they appreciate to the full the urgent needs of the medical department when thrown in the balance against ammunition trains, supply trains, etc., not only have they to work in with the medical department of the army but they must be able to demonstrate to the authorities allotting ambulance trains at General Headquarters their exact needs and get their demands complied with.

There was one court of appeal open to the Director of Medical Services if the need was extremely urgent and trains were coming very slowly or not at all into the army area. That was to telephone to the Director-General of Medical Services' office or to the Assistant Director of Medical Services of ambulance trains direct. Such an appeal was seldom made or necessary. It was always realized that ambulance trains were being distributed amongst armies as fairly as possible, still in some cases the immediate urgency could only be estimated by the Director of Medical Services on the spot.

Difficulties in obtaining ambulance trains in the army area became much accentuated during the final advance when all the armies were involved, and where it must have been an extremely difficult matter to judge the requirements of each army, particularly as armies were likely to over-estimate rather than under-estimate their probable requirements.

Where military operations partook of the nature of a set-piece, the Assistant Director of Medical Services had to calculate his requirements beforehand as regards (1) the daily number of ambulance trains and temporary ambulance trains required for the army area; this we calculated roughly for twenty-four hours, commencing from zero hours plus about eight hours. It was estimated that probably no casualty clearing station would require an ambulance train until eight hours after the action had begun, except such trains as were required to finally clear out all the sick occurring before operations.

These sick were usually gradually and unobtrusively being cleared out two or three days before the event, with a final train-load or two in the last twenty-four hours.

(2) The number of these required in the same time to each group of casualty clearing stations. It was of course difficult to make an accurate forecast but it served as a sort of basis on which the army could submit its demands. It was necessary to be careful however as in the moderation of the demands depended a good deal the confidence of the railway authorities. It was foolish to ask for five trains in twenty-four hours on a single line also heavily engaged in running up troops or urgently needed supplies. Few single lines in general use could ever manage more than three ambulance trains a day during operations. As regards (1) a better method than asking for so many trains a day was to try and arrange for so many to be put up in the army area garaged conveniently at zero hour. This was a much more comfortable arrangement and saved much anxiety for the first thirty-six hours which were usually the period of heaviest casualties. Taking an example, it generally happened that there were about three different groups of casualty clearing stations which were so situated that they would take all or the majority of the first day's casualties; one of these was probably so situated that it took the greatest part of the first rush, the other two being switched over to somewhat later; more casualty clearing stations existed in the second line, but except for special cases would not be used unless ambulance trains would not meet the demands, when motor ambulances could be directed to them, not a desirable procedure unless compulsory. In such a case a good arrangement would be as follows: have an ambulance train ready garaged at each siding for a group of casualty clearing stations within four or five hours of zero, possibly two in one group of casualty clearing stations which had extra sidings. Three ambulance trains held in railway sidings, one on the line to each group but about four hours' journey or less away (four hours on these occasions may be about ten miles away or less), two at a large junction in the back area from which they can be sent to any one of the groups which might particularly require it, and one temporary ambulance train accommodated somewhere in the army area. This temporary ambulance train was a great stand by, it took over 1,000 sitting cases, and could be put into the group where the strain was greatest; usually the

most desirable time was between zero eighteen and twenty-four hours when the walking wounded have arrived and may be blocking up a very busy group of casualty clearing stations. The Assistant Director of Medical Services is very well off if he can get all these trains and he can view the future without much anxiety. He has roughly the means of evacuating 4,500 cases for a period of twelve hours after they begin to pour in, he has his second line of casualty clearing stations to fall back on and can therefore deal with casualties up to 12,000 or more in twelve hours without being blocked,[1] as soon as the extent of the casualties begins to be apparent, there is sufficient time for fresh trains to be ordered into the army area.

The above may be considered a happy position, but it may not after all "pan out" well, the ambulance trains garaged at the most busy group of casualty clearing stations will be almost certain to get away, but it by no means follows that the next one will arrive four hours later, it may take twelve hours; even if it takes eight it upsets calculations and the Assistant Director of Medical Services will get off lightly if the worst that happens is that valuable cars have to switch off other work, or be diverted to casualty clearing stations farther to the rear, thus causing delay in clearing the wounded from field ambulances.

Other factors that have to be considered in asking for and also in estimating the probabilities of getting a certain number of ambulance trains at a certain group of casualty clearing stations are the total number of trains under best conditions that a line can take. Sometimes the number will be very few owing to heavy gradients, poor condition of permanent way, lack of sufficient sidings, etc., it will depend also on the period which has elapsed after operations have begun; usually good stocks of ammunition, etc., are already at and beyond rail head at the commencement of operations, also relief troops have not yet begun to be rushed up, so although the wounded may be in large numbers the position may be better than it will be three days later when the numbers of wounded may not be so great, yet the traffic on the line may be much increased. In fact, I have known ambulance trains held up for long periods in order to push up a large number of troop trains. This is the fortune of war.

In siting casualty clearing stations it is no good policy to locate them strung out on one railway line unless it can deal with more than five ambulance trains a day. For example, two groups of three casualty clearing stations with one group five miles in rear of the other, under the above conditions is a most undesirable arrangement. The forward casualty clearing stations with good entraining arrangements could take on all the five trains in twenty-four hours, and the strain on cars (affecting the rapidity of evacuation) would be much less than by sharing trains between the two

[1] This is of course largely theoretical; road transport to deal with such large numbers would probably not be available in that period, still the *actual accommodations* would exist.

groups. The distance of five miles is of no tactical value in case of a retreat, for if the first group were lost, the second would be in such jeopardy as to be unsafe to use; ten miles is the least distance that should be fixed between lines of casualty clearing stations when locating them with a view to a retreat. However good policy and necessity do not agree always, unfortunately.

Finally, there is the question of utilizing returning empty trains for wounded. I am thankful to say that as far as my memory serves me these were not used in our army except for very short journeys for a limited period following the retreat and in the early stages of the final advance, and then only to a very limited extent, so I cannot speak from much experience, although I saw some in use in the early period of the war. The objections to them are very great indeed, and they should only be resorted to as a last resource. Preparations to utilize them were made before the battle of Arras; fortunately there was no need for them.

The use of light railways already existing and primarily in use for carrying ammunitions and other supplies to the front for the conveyance of wounded was more the concern of the Deputy Directors of Medical Services of Corps. The Director of Medical Services took much interest in it, and very good types of converted trucks for the conveyance of wounded were evolved. On some occasions evacuation from the corps areas to one or more of our casualty clearing station groups was carried out by this means. If the course taken by the line was fairly direct they were very useful, but often it was very round about and the time involved in the journey very long. The whole business took a good deal of organization, and doubtless a Deputy Director of Medical Services, or a Deputy Assistant Director of Medical Services of a Corps could furnish much more useful information than I am able to give.

We now come to the second important subject, viz., additional transport to active operations. The usual allotment of motor ambulances convoys in the army was one for the army and one for each corps. These convoys were administered by the Director of Medical Services, and convoys were lent to the corps by him. This was an important point, as it meant that once lent by the Director of Medical Services they were under the control of the Deputy Director of Medical Services and could not be interfered with by the corps staff. This was a matter that had to be emphasized on one or two occasions. Needless to say that in active operations on a big scale, the number of cars available was inadequate unless there were one or more corps marking time on the flanks of the attack in which case it was usual to draw in fifty per cent or more of each convoy for army uses; this addition, however, was seldom sufficient. Normally about thirty per cent of the headquarter cars were on command chiefly distributed singly to casualty clearing stations or to medical officers of prisoners of war camps, etc. Wherever possible these were called in, but roughly ten per cent could not be made available. Although before a

" strafe " every effort was made to get all cars out of workshops and fit for the road, one had to allow within a very short period after operations for ten per cent being in workshops, consequently forty cars per convoy was the maximum number likely to be available, and that was a sanguine estimate. Consequently the Director-General of Medical Services generally had to be asked to provide a certain number. Thirty cars was about the maximum that could be obtained from this source, until towards the end of the war when a whole convoy was possibly obtainable except when all armies were attacking at once.

Eighty working cars was about the least number with which one could carry on, and with this number we also required additional help in the way of motor buses. These we invariably obtained, but the number varied; we had as few as three and as many as forty (not all fit for the road unfortunately). Their usefulness cannot be overestimated; generally we had at least eight, and as they hold about twenty-four sitting cases each, they could deal with large numbers. On some occasions we made up a small convoy of four or five casualty clearing station lorries for the transport of lightly wounded. As regards the allotment of duties of all these, the general arrangement was to keep fifteen to twenty cars in reserve to rest drivers and be available as a last resort, twenty cars with possibly two to eight buses loaned to corps heavily engaged, the remainder for army evacuation and isolated duties, e.g., bringing up sisters, removing infectious cases, etc. One tried to retain five buses if possible for army use. Nevertheless it may be impossible to maintain the above distribution, and I have had the headquarters cars reduced to one or two on occasions, and no further reserve to fall back on.

There are one or two points worth remembering; one is that fifteen or more cars will be employed for forty-eight hours before operations in bringing up sisters and surgical teams, etc., from all over the army area and perhaps beyond it, and so it is wise to get some additional cars in soon; another is not to forget to arrange for blankets for the wounded for each bus, to be in charge of the conductor; also as previously mentioned, interview the officer in command of the headquarters of the motor ambulance convoy and arrange in good time for him to accommodate all the extra cars and buses; if possible keep them altogether under the officer in command; it makes a great difference in the smooth running of this branch. Needless to say the officer in command must be a capable officer with good organizing powers. If the repairing outfit and personnel of another motor ambulance convoy can be obtained it is advantageous. Although it may be a good deal against the grain in view of the pressure on cars, each group of casualty clearing stations must be allotted a car during a " strafe "; in fact, if it is possible each casualty clearing station should have one. The reasons for this are fairly obvious, there are possibly local casualties to be brought in, medical stores urgently required, the officer in command may have to visit the ordnance or supply depot in a hurry, and must have means of

transport, and infectious cases have to be moved. It is best to post three cars in the group under the senior officer in command.

Extra Labour for Casualty Clearing Stations.—What one may call casual labour had to be provided for casualty clearing stations even at ordinary times, and this was met in various ways in our army throughout the war. Thanks to the amenability of "A" who always willingly assisted when the necessity was demonstrated, when casual labour became properly organized, we had allottted to us an Area Employment Company of about 400 men who were distributed among the casualty clearing stations. These were employed in stretcher-bearing duties, sanitary work, cook houses, etc. In active operations not less than forty over and above these would have to be obtained for each casualty clearing station likely to be heavily engaged. Most of these would have to be employed in stretcher-bearer duties in day and night shifts. The necessity of obtaining them should not be overlooked, otherwise the rate of unloading cars and loading trains would be so slow that the departure of ambulance trains would be seriously delayed (a state of affairs which would give rise to much trouble) and blocking of cars at casualty clearing stations would soon occur. Labour companies whose employment ceased temporarily when actual operations commenced were most usually supplied. Some of these, especially the road repairing companies, worked with extraordinary devotion to duty, others were rather "tired."

A certain number of wounded could also be employed.

Royal Army Medical Corps Personnel.—Here again increases were required in active operations but seldom as many men were available as we should have liked. These were obtained, some (not many) through the Director-General of Medical Services, others by calling on corps to provide from field ambulances of their divisions. An increase of thirty to forty to each casualty clearing station, not counting the orderlies accompanying surgical teams, was generally the most that could be managed.

There were also generally obtainable through the Director-General of Medical Services a certain number of men from the stretcher-bearer companies at the base who could be distributed to casualty clearing stations. I do not remember these at the latest period of the war, possibly they were not available owing to base requirements.

If my memory serves me right this generally brought the total personnel to between 180 and 200—exclusive of officers and sisters and local temporary men slightly wounded—for each casualty clearing station really heavily engaged.

As to the actual number required, opinions of commanding officers varied, and naturally would depend on the amount of work, its continuous nature and duration. This was not easy to estimate beforehand. Two hundred and forty was the figure given by an expert commanding officer as sufficient for all emergencies. It was not possible to supply all casualty

clearing stations equally, and in some operations the numbers per casualty clearing station were very much less, also one had sometimes to rob casualty clearing stations outside the sphere of operations in order to meet the requirements of more important ones.

Cemeteries and Burial Parties.—Arrangements for both of these may be forgotten before a "strafe." When a casualty clearing station moves to a new spot application had to be made to the Graves Registration department, either for a cemetery site to be allotted, a separate one or one for a whole group, or for permission to utilize an existing cemetery as the case might be. Usually the Graves Registration department also supplied in existing cemeteries one or two men to keep the ground tidy and in order.

Correctly speaking, burial parties should be provided by "A" branch on its own initiative and of course as regards burials in the field this was done. In the case of casualty clearing stations in active operations the Assistant Director of Medical Services had usually to take the matter in hand with "A." It is important, for if neglected a very disagreeable position arises. To dispose of an accumulation of eighty or ninety bodies or more takes a long time and a very large number of men; a group of casualty clearing stations easily gets this accumulation in thirty-six hours.

Consequently, the Assistant Director of Medical Services should see that in peaceful times a reserve of 20 or 30 graves for each casualty clearing station is kept, and get these increased by extra labour shortly before the onset of operations, so that there are at least 40 to 50 graves per casualty clearing station available at zero hour; if more all the better.

During operations a minimum of thirty men per group of three casualty clearing stations should be continually employed digging and filling in graves. "A" should furnish these men. It will thus be seen that the amount of extra labour required is considerable, and the Assistant Director of Medical Services amongst his multitudinous duties has to give time and thought to obtaining, allotting and transporting the men.

Extra Sisters and Medical Officers—For a heavily engaged casualty clearing station we trust to obtain twenty-five to thirty-nine sisters exclusive of surgical teams sisters: not that we should not have liked more but experience showed that as a rule more were not obtainable. We did not always get twenty-five especially at the end of the war. Naturally also the normal number of medical officers in a casualty clearing station did not suffice, especially as each casualty clearing station contained two surgical teams at least, which left very few medical officers for ordinary duties. Extra medical officers were obtained by drawing three from each division of a corps and from any reserve divisions coming up or waiting to be put in the line, calling in odd medical officers of units where they could be spared temporarily, such as medical officers of prisoners of war camps, labour companies, etc., and denuding hospitals in the army area outside the sphere of action.

Surgical Teams.—There was a constant flow of these from army to army and bases to army and back again according to the exigencies of the situation.

We represented our requirements to the Director-General of Medical Services and he allotted us as many as could be spared. Of course the more one could obtain the better. Obviously, however, it was not common sense to ask for say ten to each casualty clearing station engaged which was a futile demand, and would give the Director-General no accurate idea of our minimum requirements which was what he desired. Usually one asked for about three per casualty clearing station engaged. These were augmented by teams from hospital and casualty clearing stations in the army not engaged plus one or two obtained from divisions. Seven teams per casualty clearing station, with 9 or 10 to one or two especially heavily involved, was a good number to start with. Readjustments could be made later, as fluctuations in the flow of wounded to casualty clearing station occurred. The greatest number of teams ever allotted in our army to a single casualty clearing station was, to the best of my recollection, thirteen; this was of course only for a short period. If other armies were fighting we never attained such numbers as the above.

It became a standing arrangement that teams should bring a certain fixed number of instruments with them; spare operating tables may also be required and may be brought by teams; also one servant with each team should be brought as this helps the messing arrangements which are heavily strained by the sudden expansion in numbers.

Advanced Operating Centres.—These came into existence on one or two occasions in our army. There are undoubted drawbacks to them. Their utility is a matter for consulting surgeons to decide, and I do not propose to enter into an argument about them. The Assistant Director of Medical Services would have to arrange for their equipment and personnel if the Director of Medical Services decided to establish them. We had the staff and equipment of a mobile operating centre in our army for some time, but it was not used by us and was ultimately withdrawn to another army.

Walking Wounded Stations attached to Groups of Casualty Clearing Stations particularly heavily engaged.—These if well run were a very successful institution. We began them only in the last period of the war, and I am not aware if other armies used them.

This station was not a part of a casualty clearing station although it was supervised by the officer in command of one and for official purposes the wounded were shown as admissions to one of the group. The staff was made up of 1 medical officer in charge and 2 assistants, 3 nurses (or more) and roughly about 10 orderlies. The accommodation should consist mainly of 3 ordnance marquees (these are much larger than hospital marquees), 1 used as a waiting room with benches and chairs, 1 for surgery (duly equipped), 1 as a buffet fitted with chairs, tables, etc., at

which tea, soup, bread and butter, cigarettes were served out, beyond this several marquees with stretchers, etc., where men could lie down and sleep, if they wished. The station was best worked as follows, after having had their particulars taken at the casualty clearing station which was admitting at the time, the walking wounded were sent to the station, where they passed first into the waiting room, then into the dressing-room, and from there to the buffet where they were fed and then passed on to wait their turn for ambulance trains, or buses as the case might be. This wait was usually only a few hours, as the station was only in use when the work was heavy and large numbers were being admitted and evacuated. It was a particularly useful arrangement where evacuation from front casualty clearing station to rear casualty clearing station was being carried on by cars and buses, either alone, or with ambulance train evacuations as well. It required capable management, otherwise the place was soon dirty, strewn with soiled bandages, etc. If well run it was a pleasure to visit it.

Assistant Director of Medical Services' duties as regards the Provost Marshal.—Arrangements had to be made with the Deputy Provost Marshal for supplying medical officers to camps in the army area for freshly captured prisoners of war and to carry out vaccination of these prisoners. In addition traffic maps had to be obtained when issued for the operations. These maps had to be carefully considered to see that the best routes to and from casualty clearing stations were open for ambulances, and in some cases roads closed to motor traffic had to be opened to meet casualty clearing station requirements.

Arrangements also had to be made for policing special points where congestion of ambulances was likely to occur. Traffic maps also had to be issued to officers in command of motor ambulance convoys, freshly arrived motor ambulance convoys, motor buses, etc., and others concerned.

General Staff Intelligence.—Arrangements to allow them to post their own men to groups of casualty clearing stations to obtain information from enemy wounded.

Arrangements with casualty clearing stations, etc., for daily wire of number of prisoners admitted and evacuated in previous twenty-four hours (for General Headquarters' information).

Army Medical Dumps.—In active operations this was a most important matter, and had to be carefully arranged. Ordnance arrangements were such that no storing of equipment was undertaken by them in army area. The result of this was that supplies urgently required had to be obtained by the Ordnance from the base depots with the result that great delay might occur before delivery, chiefly owing to railway difficulties, also the dumping of these when they did arrive might not suit the medical arrangements. Consequently it was essential that the Director of Medical Services should arrange dumps under his own control before operations and restock them as they became depleted. These dumps as regards all articles except

stretchers and blankets were not authorized but necessity rendered the formation of them compulsory. The three chief items in the dumps were tents, marquees and bell tents, stretchers and blankets, but waterproof sheets, mattresses, eating utensils and other material *essential* to the rapid expansion of a casualty clearing station or the formation of a fresh hospital, were also maintained in some of the operations. The marquees were usually obtained from casualty clearing stations or stationary hospitals where the need for them no longer existed owing to the substitution of buildings or huts. Later on in the war, dumps of stretchers and blankets were authorized to be maintained as follows: One large dump with the Director-General of Medical Services (several thousands of both) with the Director of Medical Services (3,000 blankets, 1,500 stretchers) with the Corps (2,000 blankets and 1,000 stretchers) (this is only roughly correct, I forget the precise numbers). Corps restocked from the Director of Medical Services' dump, the Director of Medical Services from the Director-General of Medical Services' dump. The wastage of stretchers and blankets during operations was very great and largely out of control, many were abandoned, many taken and no doubt utilized by unauthorized persons. If was impossible to gather these in during operations, although steps were taken afterwards to recover as many as possible.

Casualty clearing stations running short of blankets and stretchers had another source of supply, viz., the ambulance trains. The number that could be drawn at one time from these was not great.

The main dump of the army was maintained at some central spot, usually a casualty clearing station well behind the front line of casualty clearing stations (a very necessary precaution in case of retreat, as the loss of this dump in a retreat would have been an irreparable misfortune). Shortly before operations commenced small dumps (e.g., 300 stretchers and 600 blankets) were placed at one or more of the grouped casualty clearing stations where likely to be most required and placed in charge of the senior medical officer of the group. It was advisable to maintain the army dump of blankets and stretchers well up to the authorized number, in fact, over it if possible, as the demands were very great, especially for blankets. If the operations were in winter the stock of army blankets had to be at least 5,000, especially if new divisions were being continually put into the line.

We had to lay down a rule that not more than a certain number of stretchers or blankets could be issued from any army dump without special authority from the Director of Medical Services' office. If this had not been done a Field Artillery or other unit might have cleared out the whole stock. The different uses to which the army dump was put would make interesting reading in itself. I need only say that its existence with a large amount of equipment greatly contributed in saving the medical situation in the retreat of March, 1918.

Advanced Depots of Medical Stores.—The Director of Medical Services usually inspects these before operations and ascertains generally how they stand as regards surgical material, etc.

The duties of the Assistant Director of Medical Services before and during operations regarding these were to ascertain the requirements of the officer in command (with due regard to maintaining secrecy), obtaining the stores required in the best way possible at the time (see remarks earlier in the paper), bringing up the stock of anti-tetanic serum to meet eventualities, checking the indents for these and other stores, and cutting down demands which were sometimes excessive or even preposterous, warning officers in command to be careful how indents of certain units were met during operations (some commanding officers put in demands for bandages, etc., which would have sufficed for 30,000 wounded or more), allotting areas and units which the different depots were to supply in active operations. During operations it was sometimes necessary owing to railway delays to send lorries all the way to the base to obtain surgical supplies.

The Duties of the Assistant Director of Medical Services regarding the British Red Cross Depots.—If Red Cross stores were maintained in the army area it was advisable to see the Manager regarding the position of his store from a tactical point of view. On one occasion in our area the whole of one depot, valued at many thousands of pounds, was saved from the enemy by timely warning to the Manager. Advice for the most convenient place for the depot to be situated, and assistance in obtaining site and buildings or huts. Advice as to which casualty clearing station it was particularly desired should receive Red Cross stores. Arrangements for an embargo to be placed on certain articles in general demand so that they were only available on special authority of the Director of Medical Services. For example, in one operation we arranged to reserve a number of oil stoves for Director of Medical Services' use only. We were thus able to get them issued to casualty clearing stations heavily engaged in suitable numbers. This was in the Cambrai battle which took place in late autumn. If such arrangements were not made important articles might go to the most importunate not to the most deserving.

Switching.—This term was used in reference to the system of diverting wounded and sick either: (1) from one group of casualty clearing stations to another group; (2) from one casualty clearing station to another within a group. The first was arranged in the Director of Medical Services' office, the second was a matter of arrangement between the casualty clearing station concerned; (2) was not, as a rule, interfered with by the Director of Medical Services unless confusion was occurring and upsettig admissions or evacuations. In active operations it was based on numbers, i.e., a fixed number of wounded, lying or sitting, or possibly without this differentiation, were admitted in turn by each casualty clearing station. According as the accommodation of the casualty clearing station varied, so the number fixed would differ.

From time to time during operations the number might have to be re-arranged either on account of differences in vacant beds or even on

account of the relative proportion of patients awaiting operation, or other reasons. The effective working of the switching depended greatly on the good sense and accord of the officers in command.

In quiet times casualty clearing stations generally admitted for a certain number of hours, and then cases were switched to the next casualty clearing station on the roster: (1) could not be arranged beforehand in the same way as (2). Switching depended on the "bed" situation of a group of casualty clearing stations at a particular time. If from reports of the entraining medical officer it was found that a group of casualty clearing stations was filling rapidly, and that cars full of wounded were coming up fast and beginning to accumulate—the question of switching to another group had to be considered. It was wise to act in good time, for there might already be large numbers of cars actually on the way, and wounded would have to be admitted and would overcrowd the casualty clearing station group, or they would have to be diverted on their arrival, which would entail a long journey—extra suffering to the wounded and possibly confusion at the casualty clearing stations concerned. As a rule, much saving of time and length of journey resulted if the diversion of cars took place in the corps area, that is direct from the units (field ambulances). One had to allow about two hours to get the orders for a diversion into actual operation; hence the need for timely action on the part of the Director of Medical Services. The orders on the subject to the corps concerned would vary considerably; sometimes only a partial diversion was necessary, that is, one corps area, or part of a corps area, would be involved either because these only were concerned or because it was only desired to *slow down* the rate of admissions to this particular group of casualty clearing stations.

Sometimes, depending on the situation of casualty clearing station groups as regards the routes of evacuation, the diversion could be worked in the army area by posting a man at forked roads past which the majority of cars were proceeding. He would be instructed to send so many cars in succession to one group and then switch off so many to the next group and so on. This man had to be communicated with through the senior medical officer of the nearest group as a rule. It is advisable for many reasons besides this one to have a dispatch rider attached to each important group of casualty clearing stations during active operations.

I do not propose to enter into the details of procedure with regard to N.Y.D. shell-shock cases or self-inflicted wounds. Special hospitals were selected to take these cases in the army areas. Such cases gave rise to much troublesome correspondence in the Director of Medical Services' office.

N.Y.D. shell-shock cases were very difficult to clear from our special hospital owing to the form of procedure in respect to them and to other causes. Their conveyance during active operations to the special hospital was often a strain on our ambulance resources. Field medical cards, too,

were often badly made out and gave rise to additional correspondence. After and even during operations life in the Director of Medical Services' office was made a burden by hundreds of field medical cards (A.F.W. 3118) being returned from the bases because they were incorrectly filled up.

Loss of officers' and men's kits gave rise to troublesome correspondence. Some soldiers appeared only too ready to make charges of theft against hospitals on very slender grounds. Usually the loss was found to have occurred in the field. In all cases when adverse reports were made by individuals on hospitals, etc., the matter was thoroughly investigated. In such cases it was sometimes found that the behaviour of the officer or man whilst in hospital had been bad. The Assistant Director of Medical Services, when drawing up the report for the Director of Medical Services' approval, should counter attack in these cases with all the vigour possible as one of the means of preventing officers in command of units forwarding without due consideration complaints based on extremely slender grounds. Obviously when hundreds may pass through a casualty clearing station in twelve hours evidence to confute such charges is difficult to obtain, as it is unlikely that the actions and movements of a single individual will be remembered.

The Division of Labour between the Director of Medical Services and Assistant Director of Medical Services during active operations.—There are only two alternative methods of dividing the work so as to get the most effective results. Either the Director of Medical Services must remain in the office during operations and leave the inspection of the working units to the Assistant Director of Medical Services, who reports to him, not always an expeditious or satisfactory way of getting defects remedied, or he must make the inspections himself, and leave the conducting of the office to the Assistant Director of Medical Services, outlining his policy before he leaves for his inspections and receiving and considering the Assistant Director of Medical Services' report on his return. Owing to the long distances involved and the number of units that it is essential that he should closely supervise, to try and do both would be most unsatisfactory and he would inevitably get out of touch with one or the other, most probably both. Inspections during operations are absolutely essential, it is the way in which bad organization of units, the inefficiency of officers, defective working of units and the causes, the need for alteration of procedure, can be promptly recognized and remedied. Over and above this his presence and words of encouragement to harried and overworked officers and men improve the morale all round, and instils zeal and energy into the units.

It will be seen that the second alternative which, in my opinion is the better and the one I think most usually practised, will throw in really active operations—especially in advance or retreat—a great weight of responsibility on the Assistant Director of Medical Services, and he must be prepared "to stand on his own bottom." If he has no instructions,

as may often happen when unexpected events occur, he must make his decisions promptly and carry them through. Delay or a half-hearted policy may be fatal and he will get no thanks from his Director of Medical Services for producing muddle and confusion owing to diffidence or hesitation.

I have by no means exhausted the duties of an Assistant Director of Medical Services even immediately before or during a "strafe." It has been impossible to avoid mixing up to some extent matters of procedure which the Director of Medical Services decides and the execution of the same which is more or less the Assistant Director of Medical Services' duty. Therefore it should be understood when in the above certain procedure is described it is that laid down by the Director of Medical Services and that the Assistant Director of Medical Services has the carrying out of its details.

I feel that I have written enough to give a general idea of an Assistant Director of Medical Services' duties, mixed, I fear, with a good deal of personal opinions and some irrelevances. There is much to be written about medical strategy and tactics as carried out in armies. Almost better one might call it various medical policies that can or should be adopted by a Director of Medical Services in advance, retreat, etc. Many of these are open to argument, and opinions may vary widely about them. I would mention attachment of casualty clearing stations to corps, siting of casualty clearing stations in advance and retreat, the transport of casualty clearing stations, etc. These concern casualty clearing stations alone—there are many other questions. Again there is medical policy in future wars; for example, combatant policy tends to the quickening of movement, everything and everybody will have to move more rapidly in future wars, it appears to me that we should be careful that there should be facilities for medical units to move fast or at any rate fast enough; if we cannot do so, some day we shall go back to the "newspapers' delight"—a medical scandal. With terrific efforts we moved medical units backwards and then forwards in the last year of the war. No one can maintain that the facilities for moving as regards casualty clearing stations were good enough; good fortune helped us considerably. We have much to thank the Armistice for; I have grave doubts if we could have kept in proper touch with the troops much longer. On the other hand there may be doubts how much longer the troops could have gone on at any rate at the original rate of progress.

The history of the retreat of March, 1918, as "seen" from the Director of Medical Services' office, would be a subject of considerable interest. Tragedies and comedies, successes and failures, hope and despair, captures and escapes, all mixed up together and piling up. I do not think it can be written in all its details. The personnel equation comes in so strongly. Some officers built up a reputation, some lost it. It was not always clear why an officer failed in a certain matter, he may not have been well

served, he may have been mis-directed. Some made mistakes and only their successes came to light, some the reverse. One could hardly write the account without criticizing, and blame might easily go where it was not deserved. Often one only knew that such and such a thing was not done which ought to have been, but in the turmoil one did not learn always exactly *why*. Hence until we are all dead silence is the best thing. This sounds rather "Irish," but expresses my meaning. I should not like my own mistakes shown up. One could not write this history correctly without exposing mistakes of one's own and others, and it would be a poor history that showed successes only. It would be like the biography of a General in a newspaper.

THE VISION OF THE SOLDIER, WITH SPECIAL REFERENCE TO MALINGERING.

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(*Continued from p.* 429, *vol. xxxvi.*)

III.—THE DETECTIVE.

THE detection of malingering is the only kind of professional sport in which the eye specialist in the Army can indulge. There are no rules in the game: the adept becomes such wholly through his own unaided efforts. A knowledge of men, of the broad technicalities of a wide variety of occupations and callings, a little histrionic gift in assuming in expression and manner a character in sympathy with or antagonistic to the scrimshanker—these develop a special sense which becomes intuitive. It may be the result of piecing together a host of minutiæ, comparing them, selecting some, rejecting others, till the brain acts almost automatically.

In this detective work, irksome and annoying as it can be at times of pressure, sometimes there is a compensating element. It is a question of man to man, and brain to brain. The examiner assumes disguises of face and voice in trying to plumb the depths of the suspect's mind. He may give the idea of being extremely casual, or fussy, uninterested or absurdly inattentive. Deliberate clumsiness with his papers or a sudden break-off to chat with another medical officer: these are the phases designed to distract or bewilder the suspected soldier, and to leave him in doubt as to whether the examiner is a fool to be hoodwinked or a man who stands no nonsense. Nevertheless, when the evidence is obtained, the examination is exhausting and wasteful of time. Every artifice, therefore, is justifiable to demonstrate to the would-be shammer that he is before a medical officer who "needs no ghost to teach him."

Compared with other diseases of which the malingerer makes the most, veritable defects of vision are the least difficult to ascertain because of the enormous advantage gained through the use of the ophthalmoscope. The uneducated soldier, or even the private patient, does not realize that by its means the specialist can be entirely independent of anything the patient may tell him, and his report, as often as not, will be based not upon what the soldier says he can see, but upon what the medical officer can see. In contrasting military with civilian malingering, we have to seek for the motive. In a civil case the plaintiff alleges that the impairment of his vision is due to the negligence on the part of the defendant, who may

be his employer. A full investigation, however, might reveal a very different state of affairs. The lure of damages is all-powerful, and thanks to cumbersome and leaden-footed procedure, he has time to study his case and be coached in the tests to which he will be submitted. Thus the element of surprise, invaluable in these cases of feigned visual defects, cannot easily be demonstrated in court. If he gains his case, the likelihood is that the greater part of the damages will find its way into the pocket of the disinterested attorney who so kindly volunteered to take up the case out of righteousness and philanthropy. If he loses his case, the chances are that the defendant will have the burden of costs cast upon him : if the plaintiff loses anything, it will be merely his job. The civil malingerer, besides, has this advantage that he is independent of all control. The civil examiner cannot, dare not, adopt towards him the tone that the medical officer can towards the soldier who is under military discipline, no matter what is wrong with him. The medical officer again, has this advantage over his civil colleague, that he has not to be subjected to the cross-examination of some junior member of the Bar, more anxious to show that "doctors differ" than to watch over his client's interests, oblivious of the fact that lawyers themselves, more than doctors, differ even in high places, hence the Court of Appeal, and the House of Lords.

The medical officer has to combine the functions of three separate persons in the case. As magistrate he has to sum up the evidence which he obtained as prosecutor, and if he refers the matter to a higher court, he becomes the principal witness for the Crown. In any capacity it is needless to say that he must be strictly impartial, and before he concludes that a man is malingering, he must be so sure of his grounds that he is prepared to uphold them on oath.

Many men, at a first glance, suggest malingering because of the dull, unintelligent expression that sometimes has been found in myopes, but no medical officer who values his reputation will commit himself to a positive statement until he has excluded every reasonable explanation for a man's alleged visual defect.

For military work the medical officer should have some knowledge of musketry training, and appreciate the difficulties that the recruit has in taking aim. If possible he should have his own accuracy tested by the triangle of error. For gunnery he should understand the various sights, the size of their figures, and the special work of a battery. These are practical points, and a stray question of a technical nature may hint to the soldier that further deception is unwise, and often provide a clue. Similarly a little sympathetic comprehension of some part of a man's civil occupation may divert his attention and give an opening for a surprise question.

Unlimited patience, irony, provided that it is not too intellectual, and incessant vigilance, are qualities which must be cultivated. Bluster is

always a feeble weapon, and abuse is unpardonable. A carefully worked-up show of virtuous indignation should be kept in reserve till the chain of evidence is complete.

Even though there may not be any case of suspected malingering in the day's work, advantage can sometimes be taken of the surroundings to influence the men waiting for examination. In the medical inspection room or the recruiting office seclusion is not always to be had, and a man may be examined in presence of others waiting their turn. This sometimes is helpful for it lets it be seen that no favour is shown. It enhances the medical officer's reputation when the bystanders, who are taking note of everything, see the vision of a myope, who could barely read $\frac{6}{60}$, raised to $\frac{6}{9}$, or $\frac{6}{6}$ by the first trial lens applied. It was not for nothing that the medical officer had seen the man reading a newspaper and had made a mental note of his far point. Men wearing glasses for aphakia are a godsend. Their spectacles betray the condition of their eyes, and it is somewhat disconcerting to the patient when the medical officer says across the room, and without having made any examination, "When was that eye operated upon?"

No small responsibility lies with the first medical officer who examines a man capable of malingering. If he fails to convict him, detection on the second examination by another medical officer is difficult; on the third it is almost hopeless, for the culprit, by these proceedings, has learnt the kinds of answers to give. The malingerer makes the simple statement that he cannot see certain letters. It is for the medical officer to prove that he can, and the oft-examined malingerer knows this. He can reverse the position and play with the examiner just as the first examiner ought to have played with him. How he is to be dealt with will be considered later.

Harassed War Pensions Committees, divided between ill-judged sympathy and misplaced zeal, add to the numbers of delinquents, and the perplexities of the medical officer are not unravelled by the knowledge that he is helpless to enforce punishment, far less to advise it, when a glaring case of fraud on the part of a pensioner is exposed.

Often he has to depend on the use of the *mot juste* for stopping an epidemic of malingering, and in the following case he was successful.

An ophthalmic centre was overcrowded by men from a labour unit whose trivial visual defects became alarming when a revision of categories was rumoured. No clear case of malingering presented itself till one morning when the *deus ex machina* arrived, questionably *deus*, but certainly *ex machina*, for he was brought in an army service wagon. He failed at $\frac{6}{60}$, and as a searching examination and tests discovered nothing he was put back for a further examination later in the day. This, after much self-restraint on the part of the medical officer, resulted in the admission of $\frac{6}{6}$, of which he had been certain from the beginning.

Having landed his man and obtained the evidence, the medical officer

took him to task for having wasted two hours of his time and finally asked him, "What do you think I am?" The malingerer hesitated. "I suppose you are a sort of medical officer." "No," said the medical officer decisively, "I am a detective."

As eye cases from this unit suddenly fell off, the inference is that the malingerer warned his mates that they had better not have their eyes examined up West as there was a man from "the Yard" there.

Thus an unpretentious lie eased the yoke and lightened the burden of an overworked medical officer.

IV.—Malingering as a Fine Art.

We now come to malingering as a fine art, whether in its practice or its detection.

Before we go further into the matter, however, it must be emphasized that the cases of malingering which the writer met with were found in a *civilian* army, that is, an army formed somewhat on the lines of a continental army raised by conscription, in which the able-bodied citizen had to serve his time. In this *civilian* army, comprising men from every stratum of society, were to be found those who would have been shirkers and scrimshankers in any calling or walk of life. When the law stretched a point, worn thin all but to infinity, and recognized "conscience" as an excuse for breeding disease widecast, and for ignoble surrender not of the "objectors'" homes alone but of hundreds of thousands of other homes as well, "conscience" became a proprietary article with a Government stamp marked "Duty-free," free of duty to the land of their birth.

Quite otherwise was it with the men of the old army. It has been pointed out in the first chapter that before the war, our army was composed of men who for a variety of reasons enlisted for the purpose of becoming soldiers. That was to be their profession, hence their effort to "pass the doctor" by making light of any defect of which they were conscious, hence, therefore, the silence of our text-books on the subject of military malingering. Clearly it would have been absurd to have discussed what hardly existed. But, as was said, again in the first chapter, compulsory enlistment swept into the net men who by any device or disguise, not having the wit to plead "conscience" which is concerned with the mind, fingered their bodies all over to discover some "disability." Others who had lit upon some physical defect, showed their patriotism by wandering from one recruiting station to another, and making a substantial collection of certificates of exemption, but there was nothing in these papers, or in their bodily condition, to prevent their indulging in violent Swedish exercises on public platforms or behind the footlights. If the medical officers of recruiting boards had seen some of these potential malingerers at their civil occupations they might have taken a different view of the matter.

Reservists, men of the immortal Expeditionary Force, who to their eternal glory held the line, were not frequenters of the medical inspection room. It must be made clear, therefore, that malingering as to vision in the regular army was negligible, in the civilian army it was found for the most part, if not wholly, in men who would have malingered in any case, not as a pretext but as a habit. Malingering has not died out with demobilization. It is a virus which has to be reckoned with, and its existence, morally and economically, cannot be ignored.

Although the hardened inveterate malingerer has not about him physical signs which can be interpreted as pathognomonic, nevertheless he has a bearing, an air, which those experienced in examining soldiers can recognize without however being able to describe how they do it. One man may be furtive, with a shifting look about the eyes, another may affect the smart soldier, prepared at first to brazen it out and "cheat the doctor," but this guise is difficult to keep up, and sooner or later the secretive regard is unconsciously resumed. It is one of the malingerer's characteristics that with the aches and pains of the whole kingdom of his body to exploit, often with fair success, he should choose his eyesight, the one sense that can be examined completely and independently, without reference to any story that he has made up.

He may attempt to demolish any suspicions by an innocent candour and simplicity, but this is rare and calls for intelligence. It is not uncommon for a man to say that he does not want to leave the army. He yearns for service overseas, to avenge some fictitious brother who has been "done in." This is an old trick, but the whine in the voice does not betoken wild enthusiasm or blood-thirstiness.

Every soldier who presents himself with a complaint as to his eyesight knows that he is face to face with his superior officer, and that military discipline is not suspended in the medical inspection room. Possibly this alone influences men who up to the last moment had made up their minds to "try it on," but thought better of it when they saw with whom they had to deal.

Every examination should be conducted with the utmost fairness, and even though the medical officer has his suspicions, nothing on his part should betray impatience, or lead the man to think that his statements are not believed. Nothing, however, should escape observation, for malingering may be discovered quite unexpectedly, and therefore every incident, no matter how trivial should be noted.

The man's B. 178 as often as not is silent as to his vision, it may be a temporary paper with nothing but his name. It may be covered with large rubber stamps or broad flourishing signatures which leave no room for the entering of the material points. It is of interest to state that in one B. 178 of the year 1906, there was no blank space for the record of vision.

While the man's particulars are being taken, name, rank, number,

unit, etc., it is easy to run the eye over him and to note if he answers smartly and civilly, if his uniform and person are tidy. His hands may provide a clue if, after the preliminary examination, his bearing is unsatisfactory. A dock labourer's hands are not soft, unless he has just come out of hospital before enlistment. A man who says he was a clerk in civil life has not horny hands and black broken nails, unless he had discharged his duties with such zeal as to attract the interest of the police. The civil occupation does not always bear a true relation to the actual work done. A man may say he is an optician, a calling requiring good visual acuity, but he may have been only a hand' in an optician's shop, cleaning windows, washing floors or polishing brasses. A lad with dense corneal nebulæ said he was a bootmaker, but he proved to have been employed on account of his voice by a Whitechapel bootmaker, to cry his master's goods to the passers-by. In every calling there are grades not all demanding the same visual acuity, so if the matter is of interest, the medical officer may push his inquiries. Much may be observed while asking the man about his symptoms, but once more it must be pointed out that there may be nothing against the man except that indefinable "something" which has caught the medical officer's eye. If the man has already been tested by other specialists who have failed to make anything of him, the likelihood is that he will have all his answers cut and dried and the task will be the more difficult.

The routine practice of taking the vision for distance is then proceeded with, and now every glance or movement is important. A card is placed over the left eye with its edge just touching the side of the nose. The man winces. "Surely that didn't hurt you." The man lowers his head, rubs his eyes, and is given a moment's rest. He may say that the light troubles him. Again the card is applied. He repeats questions instead of answering them, and when told to fix his attention on the test-types, stares at the examiner instead.

By this time the medical officer is not surprised that the man says he can only see $\frac{6}{36}$. At this stage the Bishop Harman diaphragm test is applied. It should be explained to the man that all he has to do is to look through the little square hole and read the letters that he sees through it. Both eyes are kept open. The medical officer covers the letters with one hand and applies the small end of the instrument to the man's upper lip. The hand is now removed and the movements of the man's head and eyes carefully watched. The moment the slightest attempt is made to turn the head to one side the apparatus is to be taken away quickly and the test begun again. If the man reads all the letters, binocular vision is proved. The test for distance is resumed and the man encouraged a little to commit himself. The head is thrust forward, the brows knitted and $\frac{6}{24}$ is spelt out slowly and incorrectly. The same result is obtained with the left eye. Without waste of time the ophthalmoscopic examination is made. The man turns his head aside, will not fix the eyes, or slowly closes the

lids. He may wince when the light from the mirror is thrown across the pupil. One man complained that the light hurt a healed wound in his arm. The remark, "Thank you; that's exactly what I wanted to know," did not convey any meaning to him, but it gave a clue where malingering was the last thing expected.

When a man is stubborn and refuses to open his eyes, or keeps fluttering his lids, it is best to say at once, and sharply, "You are wasting time. Go outside and think whether it is worth while"; or, "This examination must be made even if I keep you here over the dinner-hour." It is for the medical officer to decide, from his impressions of the man, whether it is wise to let a suspect mix with the others in the waiting-room. Public opinion—that is, theirs—may have some influence, for a party coming from some distance under charge of a non-commissioned officer may be kept waiting long after the others have been examined, owing to the recalcitrancy of one of them.

Let us suppose that the ophthalmoscope has been tolerated and that nothing abnormal has been found. The medical officer then assumes his walk-into-my-parlour attitude, and the struggle with trial lenses begins. The trial-frame is fitted with a + 10 D. for the left eye, and a + or − lens, under 1 D. for the right. The medical officer adjusts it on the man's face with his hands, covering the lenses so that nothing can be seen through them till the frame is in its place. The man is then asked to read, his eyes being always under observation. The moment an attempt is made to close one eye, the medical officer's hand should be brought up quickly so as to cut off the view. If the man with both eyes open can read better than $\frac{6}{24}$ or $\frac{6}{18}$ with the low + or − lens, the presumption is that he can get still further, the amount of improvement being out of all proportion to any refractive error present.

The medical officer having obtained a clue, can now indulge in some more encouragement by saying, "Very good," or "Perhaps I can help you a little more." Standing in front of the man he makes some play with the lenses. Never neutralizing the + 10 in the left with anything higher than a − 1 or 1·5 D., he intentionally makes a mess of things by putting up a + 3 for the right, and notes the effect. A "white" soldier, with a genuine error, generally laughs and says, "That's awful," or "Napoo," or "Wash-out," feeling that the medical officer's efforts to help him, with perhaps a word or two of chaff, have relaxed discipline somewhat. The malingerer, on the other hand, replies in a different, possibly sullen manner. He says, "No," or "No good," or turns his head away from the card. The + 3 is then gradually reduced until $\frac{6}{12}$ or better is admitted with a − 3 beside it. The medical officer may feel content with this result, or may go further. In any case he has his data.

This neutralization is the commonest test applied, and is familiar to all, but were a proficient in the art of malingering to be subjected to it, all that he would need to do, on the defensive, would be to decline to admit any improvement at all, no matter what the glass was.

There are, of course, variations. Instead of the interchange of lenses with both eyes open, the left eye may be occluded by a blank, and the right provided with a + 8, which is gradually neutralized, the amount of vision at each stage being noted as the artificial myopia is corrected.

Tests such as these rarely fail when the malingerer is a neophyte, and the result is arrived at the more rapidly when the medical officer assumes in the soldier a virtue, though he knows that he hath it not.

But should this test fail, there is nothing for it but homatropin, retinoscopy and a further ophthalmoscopic examination to see if nothing has been overlooked. The psychological effect of the mydriatic is valuable. The soldier, sitting on a bench in the inspection room, finds everything becoming more and more dim. He does not understand that the confusion of vision is only temporary. He wonders if it was quite worth while to be "up against" the examiner. He is kept waiting, he wants his smoke, he is hungry. He dimly sees, but hears others of his party spoken to with sympathy. He catches the remark made to one, but really for the benefit of himself, "You will get your spectacles, and for the first time in your life you will see the pictures," and he begins to admit to himself that he has been a fool.

When his pupils are dilated, he is taken once more into the dark room. By this time he has realized that he will not see much of the football match in the afternoon, or the cinema in the evening with his best girl, and he capitulates.

These remarks may appear far-fetched, but the habits and idiosyncrasies of the soldier are worth studying in all their aspects.

In the dark room the refraction is accurately estimated and all pathological conditions are excluded by a searching ophthalmoscopic examination. Once more before the test-types it is a relief to him to be brought back to clearness of vision by means of appropriate lenses, and after admitting his foolishness he is dismissed with a warning in soldier words.

If, however, his resistance is too strong to be broken down, it may be gently suggested to him that his vision puts him in a category as fit for labour in the line. Should this yet fail, the medical officer in the last extremity can frame his report in the following words :—

Vision, R. $\frac{6}{36}$; L. $\frac{6}{36}$. Answers unsatisfactory.
Media clear; no pathological condition present in the fundi.
Refraction normal physiologically by retinoscopy under mydriatic.
Presumptive vision normal. N.A.D.

The foregoing is an account of the usual run of cases in which a visual defect is pleaded. Many of these arise at the beginning of a recruit's first experiences on the range, and a good deal could be done to prevent attempts at excuses were a little patience exercised. Soldiering has to start with education, and despite his training, a man still retains some of his individuality, just that inextinguishable trace that nothing can drive out of him—that personal undaunted trace which on countless occasions,

unrecorded and unrewarded, expressed itself by holding the line, and a slender one it was, against open sights and machine guns.

It is difficult to keep a man under observation unless he is in hospital. His fellow-soldiers are not likely to give any information about him unless he is unpopular and is a drag on the all-round efficiency of his company. It must be said that hopeless cases of incorrigible malingering are extremely rare. It is the attempts that occupy so much attention, due as much to ignorance and an entire change of environment as to deliberate wilfulness, and a little heart-to-heart talk can shape unpromising material into good soldiership. Everything, however, depends upon those into whose hands the recruit passes at the outset of his career in the Army. That malingerers do not turn out badly is shown by the following. About the time of the Armistice, a man was sent up by a Dispersal Board for a report on his vision. One eye had a ruptured choroid due to a wound received in action. The medical officer recognized him as a man whom he had examined two and a half years before. The man had then been only ten days in the Army, had a low refractive error for which he was wearing glasses, and pretended that his unaided vision was only $\frac{6}{24}$. It actually was $\frac{6}{9}$ unaided. No doubt he concluded that it was not so easy as he thought to deceive the medical officer and adopted his suggestion that his better course as a soldier was to be straightforward. He went to France, and returned with an honourable wound-stripe.

We now come to malingering among men who wear glasses. These for the most part are myopes who are conscious of their defect, and want to make the most of it. The spectacles should be verified. They may be a compromise, there may be an axis misplaced or some error in the strength of a component lens. If the revised correction does not help, the man should be told that the new spectacles are consistent with his being able to see up to a certain line. This generally has the desired effect, for a myope with the wrong correction or without glasses at all, will constantly be in trouble for not recognizing and saluting an officer, or for failing to perform duties for which a fair amount of vision is indispensable. In the case of myopes, difficulties are sometimes created by practitioners who in good faith, and ignorant of army requirements, assure their patients that they are certain to be rejected on account of their eyesight, and a certificate or letter to that effect may be forthcoming to anticipate and discount the opinion of the regimental medical officer.

A protracted examination may lead an ignorant man to think that there is something seriously the matter, when possibly there may be present an unusual congenital appearance without the vision being impaired. Thus the knowledge that there is something interesting in his eye may suggest a dishonest course of action, and when examined by some other specialist, he may be disposed to enlarge upon and trade upon the abnormality.

The question of spectacles for musketry will be discussed when we come to consider as a whole the issue of these appliances.

Blepharospasm is sometimes assumed, especially by miners who have picked up and tortured the technical names of eye affections due to their vocation. In these cases an ophthalmoscopic examination is resisted, but the pretence cannot be maintained for any stretch of time, and will be discovered when the man is off his guard. The depth of the wrinkles on his forehead may afford evidence as to whether the condition is assumed or genuine.

An artefact conjunctivitis is produced by introducing an irritant into the lower cul-de-sac. The effect is local and the absence of a general injection of the vessels will cause suspicion, especially when the upper lid is everted.

The commonest applications are soap and tobacco juice. The head of a match rubbed inside the lower lid or a hay-seed retained between the lids will set up the irritation. In one case, a piece of plaster measuring 6 or 7 millimetres by 5 was found under the upper lid. The pain produced was intense.

When several men in one unit, are found suffering from conjunctivitis, artefact should be suspected, for this form of malingering can be epidemic, as was found in one part of the line in France (it was not British) at a certain stage of the war. The irritant was ipecacuanha powder.

Isolation and vigilance will decide the case, which, if proved, should be dealt with promptly.

It is extremely unlikely that a man, knowing the risks, would deliberately infect his eye with gonorrhœal discharge, though he might have no scruples about applying it to his meatus. Such a case would call for the most minute investigation. The writer met with only one case of gonococcal infection of the eye in four years in the Army, and it was proved beyond doubt that the man had contracted the disease innocently. The source of infection was discovered and the eye was saved.

(*To be continued.*)

THE PRACTICAL PREVENTION OF TYPHUS FEVER AND RELAPSING FEVER IN MESOPOTAMIA DURING THE WAR.[1]

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BEFORE commencing a detailed study of the occurrence and practical prevention of typhus and relapsing fever in Mesopotamia, it will be helpful to outline the conditions under which the campaign was conducted.

The inhabitants of Mesopotamia during the war may be divided for epidemiological purposes into those imported (British, Indians, Chinese, Egyptians, West Africans, etc.), and those who ordinarily lived in Mesopotamia (Arabs, Jews and Persians). The native civilian population lived in the town in brick native houses, and in the country in reed and mud huts, whilst a large proportion, being nomads, lived in sacking-tents or on native river craft. The troops and labour corps were either in bivouacs, tents, or billeted in native huts.

The distribution of the population falls into three groups. The first group included advanced troops, troops on the march and in outlying posts, Turkish prisoners, and natives of recently occupied villages. The second group, on the lines of communication, consisted of troops doing guard or transport duty, Arab, Egyptian and Persian coolie labour corps, and natives in villages and towns. Finally there existed a large group of troops and labour corps, either stationed at the base or passing through. In each group lice-borne disease occurred and each required the adoption of different preventive methods.

It must be remembered that the condition of warfare in Mesopotamia, especially as it affected the disposition of troops, differed very materially from that of European countries, and occasioned different problems of typhus prevention. In France, Serbia, etc., with a definite front line, and the slow progress of trench warfare, it was generally possible for troops to return from the line to rest camps, and very often to a rail head, allowing for the easy transport of disinfecting apparatus, etc. In Mesopotamia, the campaign consisted of a series of engagements, often between very mobile troops, whilst the largest proportion of the troops was engaged in holding long lines of defences, completely isolated from other units, except for rationing; and not returning to the advanced bases, rail-heads or main lines of communication, for six or eight months at a time. Such lines as the Euphrates defences, Persian front, and front line west of Baghdad,

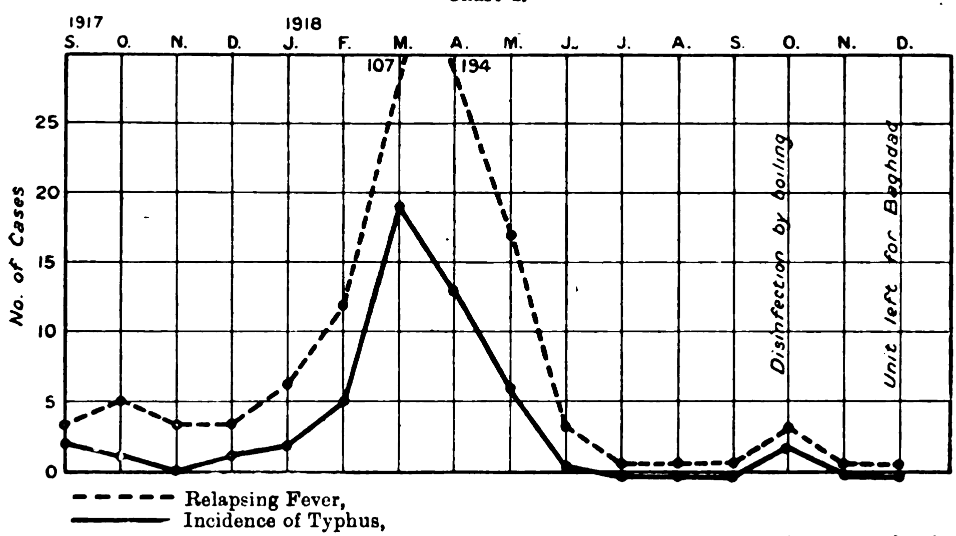
[1] Thesis approved for the degree of Doctor of Medicine in the University of London.

were each as much as two hundred miles in length. The problem was further complicated by the presence of large coolie construction camps on road-work, etc., in the desert. The posts on these defences in the case of troops would consist of perhaps a battalion, and at intervals a complete flying column, of two or three thousand men. The coolie camps contained from two to four thousand men. Further the troops were much more mobile than in France, and complete divisions and coolie-corps spent weeks and months on the march, from post to post, or from one point of construction to another. At certain centres on the river, especially the advanced bases and the main base, troops were closely camped together and, as far as the transmission of infection is concerned, in constant contact through reinforcements, transport of rations, and material, with the large outlying units. In such areas eighteen to twenty thousand men would be living in addition to the civilian population.

At first it might be thought that the isolation would have automatically "ringed round" outbreaks of typhus or relapsing fever, but practically this was not found to be so. Chart I shows the incidence of an outbreak of relapsing fever and typhus in one such outlying community. I attribute such outbreaks to one of the following reasons. In the first place constant communication had to be maintained between all units for rations, material, etc. This was undertaken as far as personnel was concerned by Persian and Arab coolies, who were invariably extensively louse-infected and consequently offered a ready means for the spread of infection from unit to unit. Further, the cases of illness in order to reach an infectious hospital had often to pass through and stay the night in, areas occupied by troops and large coolie corps, the patients travelling long distances by camel, motor ambulance, or launch to the hospital. It might be urged that cases occurring in outlying units should, therefore, be treated by that unit. This was considered, but was impracticable owing to the shortage of medical personnel which did not allow of a medical man being attached to the majority of these corps, the necessity of a microscope for diagnosis, and finally the impossibility of providing intravenous treatment. One of the most prolific causes of localized epidemics was undiagnosed cases. A coolie would report sick, and owing to the fact that no microscope was available, the case was diagnosed pyrexia of unknown origin, and sent down the line to hospital. By the time the man reached hospital his fever had subsided, and diagnosis was impossible. After a few days in hospital he would be discharged and returned either to his own unit or to another. Several such attacks might occur before one attack of pyrexia coincided with his being in hospital. Such a case would leave in its wake a train of infected louse communities, each a potential start for an epidemic. Further a certain number of ambulatory cases occurred and were most difficult to detect. Finally, as a cause of outbreaks in isolated and other units must be mentioned, the failure of such machines as Serbian barrels, Thresh machines, etc., to effectively kill eggs of lice in practice, a subject

dealt with later. It is interesting to note the two curves of infection in the corps incidence shown in the chart. It will be noted that during the greater part of the winter, cases were comparatively few, but occurred constantly. Then suddenly in the same corps an extensive epidemic occurred involving thirty per cent of the strength. The regular occurrence of a few cases in a corps was generally due to a fault in the method of disinfection, adopted by the unit, or lack of discipline when carrying it out, enabling a few men to escape. This succession of cases was generally stopped by thorough disinfection. The sudden outbreak I attributed to the influx of infected lice with a new draft of coolies, from an infected corps, or from Persia. As will be discussed later such drafts were continually arriving in uninfected units.

Chart I.



In No. — Persian coolie corps during the period September, 1917, to December, 1918, showing typical smouldering case incidence in an outlying unit, with an epidemic in March and April, and the complete disappearance of the disease during the summer and for two months after complete disinfection by boiling.

Looked at broadly the infectious diseases of Lower Mesopotamia may be divided into those that are endemic and those which are not. Consequently it is important to consider carefully the probable origin of the disease. Small-pox, plague and, probably, cholera are endemic and occur to a varying extent throughout the civilian population in river-side towns and in remote villages isolated from trade routes. I found no reason to suppose that either typhus or relapsing fever is endemic in Lower Mesopotamia. My work in searching for cases of infectious endemic diseases took me into villages far remote from intercourse with trade routes. In no case did I find a case of typhus or relapsing fever in a village unless recruits had returned to that village from a labour corps.

Typhus and relapsing fever constantly disappear during the heat of the summer in Mesopotamia. The comparative coldness of Persia, Turkey, and Afghanistan allow lice to persist throughout the summer, whilst it is impossible to find lice in Mesopotamia after June. The probability is, that not only has lice-borne disease to be re-introduced every autumn, but that the insects themselves must be conveyed to the country when the lowered temperature at the end of summer allows them to exist. In this I am at variance with Colonel James who states that the disease, and presumably the lice, are endemic in Lower Mesopotamia (*Trans. Soc. Trop. Med. and Hygiene*, November, 1918). The infection of the community with lice might occur along almost any route from a colder country, e.g., France, Northern India, etc., whilst the infection with the disease occurred along two defined channels. The commencement of the outbreak of lice-borne disease in the winter of 1916 in Lower Mesopotamia can be definitely traced to the advent of a party of 800 Turkish prisoners, which arrived from up the line, suffering from this disease, and before whose arrival the disease had not occurred. Infection then came from Turkey and Asia Minor, where lice-borne disease is endemic, and infected the louse-community of Lower Mesopotamia, outbreaks occurring thereafter regularly, until the summer. In the autumn of 1917 and 1918 the disease was re-introduced by a new channel. Large labour corps were at this time being recruited from Persia, through Bushire. By the terms of their contract the coolies served times, varying from one to six months, in Mesopotamia and were then returned to Persia. This necessitated a constant traffic in thousands of lice-infected coolies, between Bushire and Lower Mesopotamia. As the voyage to Basra only occupied three days, a man infected with the disease in Persia would develop it after arrival in Basra. As a port health officer, I met and examined such coolie ships regularly, and frequently detected cases of relapsing fever amongst the coolies on board. Further, in the examination of between four and five hundred ships arriving from India, during the winter, I failed to find any case of lice-borne disease among the sick on board. The endemic centres in Northern India may therefore be excluded as an origin of the disease, and we may say that whilst in 1916 lice-borne disease in Lower Mesopotamia was of Turkish origin, it was in 1917 and 1918 in all probability Persian, the front line having advanced beyond Baghdad, which now became the base, where all Turks were disinfected before reaching Lower Mesopotamia, the Turkish channel of infection therefore ceasing to exist.

With regard to the transmission, as far as could be observed, the main agent appeared to be *Pediculus corporis*. The bed-bugs (*Cimex lectularius* and *C. rotundatus*) are not found in Lower Mesopotamia.

With respect to racial predisposition, no race was immune. Lice infected from and living on Persians or Arabs, would spread the disease readily to either negroes or Chinese camped next to them. As far as my experience goes the infected louse-community fed indiscriminately

from black, white or yellow skins. Children were relatively immune. Cases were absolutely rarer amongst Arab children, who greatly preponderated, than amongst Persian children.

Mesopotamia offered a special opportunity of observing the relation of typhus to famine and mental depression. Mr. James Berry (*Proceedings of the Royal Society of Medicine*, 1918), Miss Robertson (*ibid*, 1917), and Soubbotitch (*Proceedings of the Royal Society of Medicine*, 1918), lay stress on the close connexion of lice-borne disease with famine. Soubbotitch says: "Does malnutrition operate directly by increasing the opportunity of men getting infected with lice, etc., or indirectly by making men susceptible to doses of infection, which could have had no effect had their nutrition been good? The association of relapsing fever with famine is so close that the latter explanation suggests itself for that disease." It is interesting to note that in Lower Mesopotamia generalized famine was unknown. The coolies were drawing very much higher pay than ever before in their lives, and were in addition supplied with a full Government ration; and yet certain corps had an incidence up to forty per cent of their strength of relapsing fever and typhus. This bears out rather the first of Soubbotitch's alternatives (cf. Rathlin Island in 1817 when typhus raged on the mainland, the disease did not occur on the island—though famine was present in both places). Nor could I trace that mental depression *per se* had any direct influence, as even amongst the most heavily infected coolie corps happiness and jollity were invariable amongst the healthy. In Mesopotamia the epidemic had no connexion with either famine or mental depression.

In combating lice-borne disease, especially in its minor epidemic form the aim should be the destruction of every infected louse, and not the mere reduction of their numbers. There are three possible positions for the insect, viz., firstly, on the body of the host; secondly, in his clothing; and, thirdly, in his billet or tent. Most careful search of quarters recently inhabited by troops or natives known to be louse-infected, has invariably failed to find lice except on cast-off clothing and sacking; nor in my experience has clean clothing become louse-infected when introduced to a dug-out or hut. Experimental examinations comprised the interiors of dug-outs, reed-huts, and tents, and not only were both woollen and cotton clothing introduced for varying times, but the floor, tent, or tent-walls were systematically wiped with both moist and dry cloths, which were examined before and after incubation.

I would here state briefly some of the difficulties peculiar to the Mesopotamian campaign which complicated the problem of preventing lice-borne disease. The climate during the winter after the extremely hot summer encouraged men to put on excessive clothing, to wash as little as possible, and to huddle together in their quarters. The medical officers, who were chiefly sent out direct from England or France, had few of them seen typhus or relapsing fever. Consequently early cases of typhus or

relapsing fever in a unit, the diagnosis of which is of the utmost importance, were overlooked frequently, and not until an extensive outbreak occurred in the unit was the disease recognized.

Further, the fear of hospital inherent in natives and coolies led to the sick hiding themselves. Especially did this occur on board ship amongst the coolies working the cargoes at Basra. On several occasions during organized search for cases that were hiding during cholera and typhus outbreaks I found coolies dead or moribund from one of these diseases in remote corners of the ship. The reason for the search generally was an outbreak of one of these diseases in a coolie-unit, apparently sporadic, but probably due to the dead coolie. Added to this, weekly disinfection was carried out very ineffectively by units, the method employed (*vide infra*) being at fault in most cases; whilst further, that dangerously erroneous idea prevailed universally that if one-third of a unit were disinfected each week, all lice must be killed in that unit. As will be shown later, it is of the greatest importance if relapsing fever is to be prevented that an absolutely effectual method of louse-destruction be evolved, and that further it is essential for a unit to be completely disinfected in one day. Otherwise not only do the louse infested infect the clean, but it is impossible to insure that every man is disinfected (*vide infra*), especially in such partially disciplined corps as labour and coolie units. In consequence, lice-borne disease of both kinds was occurring regularly throughout the winter, raging in some units and districts as a widespread epidemic (forty per cent of some units being affected), whilst in others a regular crop of cases occurred each week throughout the cold weather.

The area sanitary officer is the most important person in typhus prevention. He should have had extensive experience at a hospital for infectious diseases. It is well too, that he should learn at once the enormous importance of early diagnosis and early disinfection, and that he adopt a method that will invariably destroy all lice that can be infected.[1]

The early diagnosis of typhus is of great importance. It is often mistaken for influenza when the first case appears in a unit at the beginning of the cold weather. Diagnosis depends on the presence of catarrh, the leucocyte count, and on the clearer mental condition of the influenza patient. The diagnosis of cerebrospinal fever is often made in a case of typhus. This tendency is partly due to the fact that a modified Kernig's sign occurs in typhus, and partly because the cerebrospinal fluid is under pressure when lumbar puncture is performed. Cases were sent to the infectious diseases hospital with a diagnosis of cerebrospinal fever on account of headache, Kernig's sign, and the marked pressure of the cerebrospinal fluid. It is easy therefore, unless meningococci are definitely found,

[1] In order to provide for the early recognition of sporadic cases of lice-borne disease, classes for medical officers were arranged at the Isolation Hospital, by Lieutenant-Colonel F. E. Fremantle, when instruction was given in the diagnosis of infectious diseases.

for the cause of an eventual outbreak of typhus to be overlooked and no due precautions taken. A prodromal rash in small-pox may closely resemble that of typhus, and even when seen side by side on stretchers the rashes are most difficult to distinguish. A recent well-marked vaccination is an important point in differentiation. The diagnosis of typhus is often exceedingly difficult and sometimes impossible, and it is a most valuable rule to treat any suspected case as typhus and thoroughly disinfect. This is of great importance in Mesopotamia where the disease disappears in the summer and the first few cases appearing in the autumn are easily overlooked with most serious results. It is essential therefore, to keep a most careful watch for the appearance of lice-borne diseases directly the insects themselves appear after the summer, so that any outbreak may be immediately "ringed round" and the discovery of the channel by which the infection has entered the country facilitated.

In considering the detailed steps adopted it is convenient to revert to the classification given under distribution earlier, viz., advanced areas, areas on the line of communication and the base area—each of these will be dealt with separately under the headings *A*, *B* and *C*.

(*A*) The prevention of lice-borne diseases in advanced and outlying areas comprising troops on the march, units occupying outlying posts, consisting of one to two thousand men, large labour coolie camps of 3,000 to 4,000 men, Turkish prisoners, and natives of recently occupied villages—units scattered at distances varying from three miles to thirty miles along a front of perhaps 200 to 300 miles and in constant epidemiological connexion with other units for rations, transference of sick, and war materials, outlying units often only being reached by passing through other units.

In the case of Mesopotamia where, though lice-borne disease was not endemic, yet the climatic and campaigning conditions in winter greatly favoured an outbreak of lice-borne disease and where a constant influx of Turks, and later Persians provided a constant source of infection, it was early decided that the special conditions called for the introduction of a new form of prevention, unless epidemics were to occur regularly on varying scales as hitherto. Once a unit becomes infected the aim must be not merely to delouse the unit in a general way but to evolve a method that will invariably kill every louse and egg in that unit and so to organize the disinfection that it is impossible for any individual of that unit to avoid the process. Further, it is obvious that unless delay is to occur, each unit must not only have its own disinfecting plant as hitherto, but that plant must invariably kill lice-eggs even when worked by the untrained personnel of the unit. Furthermore such a method must provide for the delousing of as many as 3,000 men per day, to allow of a unit disinfecting itself completely in a day, and to avoid the most unsatisfactory practice of disinfecting a unit in sections on successive days. Caste, too, must be provided for, to enable both sepoys and sweepers

in a regiment to be dealt with on the one day, and the apparatus, as the troops were mobile and advanced, must be as simple as possible, light and not highly specialized. Every unit must, on receipt of a wire from the infectious diseases hospital, have a method of self-disinfection which when carried out by themselves would invariably destroy all lice and eggs in the community, avoiding the false sense of security that other less effective methods were giving to the medical officer and the commanding officer of the unit.

The broad method to be adopted was that of stopping the influx of infection to Lower Mesopotamia by segregation of Turkish prisoners and Persian coolies (the latter owing to the great scarcity of labour was not always possible) and further the "ringing round" of sporadic outbreaks or more commonly (from late or wrong diagnosis of the initial cases) of localized epidemics, by a thoroughly effectual method. Coolies and Turkish prisoners were segregated on arrival, unless, as frequently occurred, the urgent need for labour prevented this very desirable step from being possible. In the latter case a wire was sent to the medical officer of the unit to which the draft was proceeding, informing him of the danger.

Let us critically review the methods hitherto employed in Mesopotamia for louse destruction and the dangers attached to the "routine disinfection of units."

Serbian barrels first used in Servia by Colonel Hunter and Lieutenant-Colonel Stammers were in use by many units. These, as actually used in the field generally failed to kill lice-eggs. Though eggs are killed when the barrel is working well with trained men, it is found in practice that as one goes round a camp that is being deloused by Serbian barrels, in the great majority of cases eggs placed on the top layers of the clothing in barrels are unaffected and hatch out after incubation. This is borne out by the fact that in many units in which cases of relapsing fever were occurring regularly, perhaps six or eight a week, it was possible to cut short the disease absolutely when a more satisfactory method was adopted. Further the Serbian barrel (sixty gallons capacity) deals only with ten blankets and one full kit (including spare kit) in an hour; thus to disinfect a unit of 1,500 men, 200 Serbian barrels working for seven hours would be required. The large amount of handling of infected clothes with the danger to the personnel, the time (one hour) required for each group of kits increasing the difficulty of maintaining complete separation of deloused from the louse-infected men, the impossibility of completely disinfecting the unit in a day with a reasonable number of barrels, their weight in the long transport to outlying units or for mobile units, and lastly their ineffectiveness in practice made their use under the conditions of the Mesopotamian campaign undesirable.

Many units placed reliance in *Thresh machines*. This apparatus has many of the practical drawbacks of the Serbian barrel and in addition it is very difficult to move without damage on rough desert transport roads

owing to the frequent breakage of the wheels. Both the Serbian barrel and the Thresh machine should be looked upon as palliative and as useful only when the reason for delousing is the comfort of the men or when an extensive and wide-spread epidemic is raging and more than a diminution of the disease cannot be attempted.

I carried out a series of experiments with *sulphur dioxide* as the lethal agent. In the first case a hut (twenty feet by twenty feet by twenty-two feet) was built of dried mud and reeds. This was made gas-tight, the clothes hung on lines stretched across the hut, burning sulphur introduced and the hut sealed up with mud. In a further modification I used the hold of an iron barge which was connected with a portable 2¾ horse power Clayton sulphur dioxide disinfecting engine. The clothes were placed on lines in the hold, the hatches closed and the Clayton apparatus set in motion until a high percentage of sulphur dioxide was found in the hold on testing specimens of the air by the absorption method. Both methods were in practice unsatisfactory. The eggs were often not killed, the time occupied in arranging the clothes on the lines with the great danger to the personnel employed and the length of time before the clothes were available for issue to the men made this method impossible. We may therefore exclude sulphur dioxide as a practical lethal agent.

Ironing of clothes, though most satisfactory in its lethal effect, has great practical drawbacks. To iron a single uniform completely and efficiently requires in practice five minutes, and this for a battalion means the employment of a very large staff of men, if the unit is to be done in a day. This in itself offers very great opportunity for scamping, with consequent re-infection of the unit with lice almost immediately. Again a false security is engendered.

In Mesopotamia I tested specimens of the commonly used *Insecticides* on lice and their eggs, both in the laboratory and in the field with such powders scattered on the clothing of the men. Practically it is impossible to get men to scatter the insecticide efficiently in their clothes. Moreover the effect of insecticides in the laboratory is very unsatisfactory, many specimens failing to kill lice, much less their eggs. The insecticides tested included naphthalene, camphor, iodoform and several well-known makes of insect powder. (Cf. Jackson and Castellani in *The Journal of Tropical Medicine and Hygiene*, November, 1915.)

The conditions required in a method of delousing applicable to a country where the disease is not endemic are different from those of a country in which the disease is "endemic." In the former it is desirable to "ring round" outbreaks with an absolutely efficient ring of disinfection, rather than to attempt a generalized and partial delousing of the whole district. In a campaigning army in such a country as Mesopotamia it is impossible to kill all lice, and almost universal lousiness exists. As far as serious epidemic disease is concerned it is useless merely to

reduce their numbers but it is practicable to "ring round" outbreaks with absolute destruction of all infected lice. This is the line on which we should work in a country in which the disease is not endemic rather than attempt incomplete disinfection on a wide scale. If both policies are possible they should be adopted but the conditions of war generally prevent this. In districts when the routine disinfection of corps by sections, each week with unsatisfactory apparatus, was replaced by a method of absolute destruction of all lice, directly a sporadic case or an epidemic occurred in the unit, a greatly diminished incidence of lice-borne disease generally, as compared with the results under the older policy of routine disinfection, resulted.

TABLE I.

Unit	Strength	Boiling vats	Baths, canvas	Serbian baths as available	Oil cookers, etc.	Chimneys	Remarks
30th Persian Labour Corps	500	1	1	..	2	1	
21st Arab ,, ,, ...	500	1	..	..	2	..	
22nd ,, ,, ,, ...	800	2	1	..	4	1	
37th Persian ,, ,, ...	700	2	1	..	4	1	
I.W.T. R.E. N. Magil	4,000	6	2	..	12	2	
Railway Shipping Officer's Camp ..	500	1	..	..	..	1	
Ordnance Depot	900	2	1	..	4	1	
Railways	2,000	3	1	..	6	2	
Sanitary Section	120	2	1	..	4	1	
6th Labour Corps	1,400	2	1	..	4	1	
P.C. and C. S. Magil	900	2	1	..	4	1	
10th Dis. Porter Corps	1,400	2	1	..	4	1	
30th Persian Labour Corps (Coal Island)	200	1	..	..	2	1	
No. 4 Works Company	700	2	1	..	4	1	
Port Traffic Labour Camp (Chinese Section)	..	3	1	..	6	2	
Labour Camps, Gurmat Ali Bridge	400	1	1	..	2	1	
Gurmat Ali Brickfields	400						
P.C. and C. N. Magil	900	2	1	..	4	1	
Base Supply Depot	300	1	1	..	2	1	

The method to be generally adopted throughout a district must be formulated early in the summer, so that sanitary officers can submit the requirements for units in their area for the following winter. It is then possible for each unit to have at the end of the hot season a thoroughly effective disinfecting station, which is mobile if required, which has been approved by a sanitary officer, and which on receipt of a wire that lice-borne disease has been recognized in one of their sick in hospital, can be immediately utilized. Each unit was visited during the summer, a scale of articles required drawn up (see Table I) and a second visit made at a later date to see that each unit had an effective disinfecting station, barbed wire for enclosures, baths and oil-cookers (vide infra). This was of particular importance in outlying units. When a case or series of cases of typhus or relapsing fever occurred, notification was received by

wire from the hospital to which the case was sent, to be confirmed later from the Infectious Diseases Hospital. The importance of immediate action in a non-endemically infected country cannot be over-estimated, as every hour means further "contacts" and the possible infection of other units. To wait for a trained sanitary staff to go down perhaps one or two days' journey to disinfect the unit is dangerous. It is essential, therefore, for each unit to have its own disinfecting station which it can use itself (i.e., is fool-proof and efficient) and which is immediately available for use on receipt of a wire, thereby avoiding delay.

Sporadic outbreaks and localized epidemics were continually occurring, too little importance often being attached to the lice that escaped destruction, either on account of an inefficient method or lack of discipline, and which served to infect other units and to cause recrudescences in the original unit. Experience shows that weekly routine disinfection by units themselves by the ordinary methods in unskilled hands is very slipshod, and is, therefore, highly dangerous once the unit has become infected.

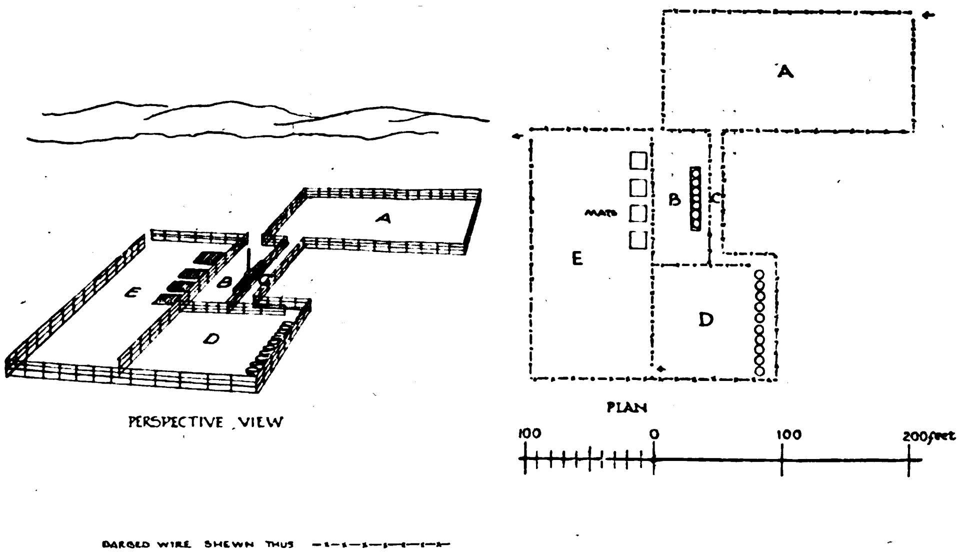


FIG. 1.—Mobile disinfecting station for advanced and outlying units supplied to Magil Area, Euphrates Defences, etc.

The most practical and thorough medium for units in the first group (*A*) is the immersion of all kit and clothing in *boiling water for* $1\frac{1}{2}$ *minutes*, and discipline and disinfection can only be insured by the use of barbed wire cages run up temporarily (see fig. 1).

Immersion in actually boiling water, especially after the *addition of alkali* invariably kills lice and eggs in one and a half minutes. The method enables the commanding officer of the unit or a political officer, to have his own station, in the event of an outbreak of typhus or relapsing

fever in his unit, it requires a minimum of apparatus, works rapidly, so that if necessary, 4,000 men can be deloused in a day (this was regularly done, when required); combined with a barbed wire construction as described below it makes the escape of any individual from disinfection impossible. It is readily modified to provide for caste differences, thus enabling both sepoys and sweepers of a regiment to be disinfected on the same day ; it can be readily and effectively worked by the untrained men of the unit, and finally it invariably destroys both lice and their eggs. The objections that may be urged against boiling are threefold,—firstly, the shrinkage of woollen garments, secondly, the difficulty of maintaining the water at a vigorous boil, when a large number of kits are immersed, and thirdly, the question of drying the clothes.

(*To be continued.*)

CONCERNING A FREE-MARTIN.

By Colonel Sir ROBERT FIRTH, K.B.E., C.B.

A friend of mine who goes in for breeding cattle showed me recently one of those mysterious and somewhat uncommon quadrupeds known as free-martins. The animal in question was an apparently sterile cow born co-twin with a potent bull. My friend, who is a very intelligent layman, amused me much with his comical explanation of the unwonted birth of the animal; into those explanations I need not enter, but, having dabbled a little in genetics, I was able to give him a more scientific explanation of the occurrence, and as the matter has a general bearing on the theory of the differentiation of sex it may be of interest to readers of this journal.

To begin with, I premise that the reader is more or less familiar with the Mendelian phraseology, and, therefore, would say that a free-martin is a pure or extracted recessive in respect of its genital determinants, and that the potent twin is a pure or extracted dominant, both being of F^2, in the Mendelian scheme. If the reader be unskilled in recent embryological theorizing, this statement may seem not very helpful, but it will be more comprehensible if it be borne in mind that there is reason to think that, in humans and the vertebrates generally, the female and the male have each not only characteristic sex organs fully developed, but also traces of those of the opposite sex. The conclusion is drawn that the ova and the spermatozoa are gametes, each of two types; thus there are two male gametes of unequal value and two female gametes also of unequal value. One male gamete contains the male sex-unit character, and one female gamete has the female sex-unit character; consequently we may speak of a male sex gamete and a male non-sex gamete, and of a female sex gamete and a female non-sex gamete. A further hypothesis is that the crossing of a male sex gamete with a female non-sex gamete will give a fertilized ovum or zygote, which will develop into a male holophyte or fully developed organism, and that the crossing of a female sex gamete with a non-sex male gamete will give a female holophyte—the sex being determined and unalterable as soon as the zygote is formed.

According to modern teaching, in the male genital unit there are potent and dominant male elements, as represented by the testes, the phallus, prostate and prostatic urethra, scrotum, Wolffian ducts and Wolffian bodies, the gubernaculum and the caudal ligament of the testis; while the non-potent or recessive elements in the male are the mammæ, the prostatic utricle, fimbriated ends of Fallopian tubes and the paradidymis. The female genital unit is also represented by potent or dominant female elements and non-potent or recessive female elements. Among the former are the mammæ, ovaries, ovarian ligaments, Fallopian tubes, uterus, vagina, hymen and urino-genital sinus. In addition to the foregoing, the round

ligament, labia major and minora, and the glans, are potent and dominant female elements, less fully developed than in the male, but always analogous to a definite continuous segment of the corresponding male organs. On the other hand, the epoöphoron and upper portion of the Wolffian duct, which are equivalent to corresponding fully developed organs in the male, but less perfect in structure, and not equally functional, are regarded as non-potent or recessive female elements.

The practical interpretation of these genetical hypotheses or views is that a free-martin is not a hermaphrodite, but may be one of two types. If born co-twin with a potent bull, such as was the beast belonging to my friend, it is really a sterile male, and, according to the Mendelian scheme, would have originated in the following way. A male sex gamete united with a female non-sex gamete and gave a zygote F^1; this, owing to it twinning, gave a bull with equivalent soma and dominant or potent genital organs and a bull with equivalent soma and non-potent genital organs of the female and recessive type. These twins constitute F^2 in Mendelian language, and were the potent bull and the free-martin respectively, the former being a pure or extracted dominant and the latter a pure or extracted recessive and really a sterile male. The other and much rarer type of free-martin is the one born co-twin with a potent cow, and which really is a sterile female. The originating elements in this case being a non-sex male gamete uniting with a female sex gamete. Apart from the question as to how free-martins originate among cattle, the hypothesis of varieties of gametes and the distinction of the genital organs into dominant and recessive or potent and non-potent parts or representatives affords an explanation of the presence in one sex of rudimentary organs of the other as well as of the occurrence of the strange structures known as teratomata or embryomata. The view held generally is that a teratoma arises from a non-sex variety of gamete, that is, that the testicular teratoma is derived from an imperfectly reduced non-sex male gamete, and the ovarian variety from an imperfectly reduced ovum. Obviously, the whole subject is both intricate and difficult, requiring close study and a working knowledge of Mendel's law in all its ramifications; but it would seem that, little by little, some of the most mysterious phenomena of generation and its anomalies are being better understood, and that there is a reasonable hope of disorder being reduced to order and an explanation forthcoming of many matters regarded hitherto as inexplicable. Any one wishing to pursue this subject further will find a suggestive article on differentiation of sex by Berry Hart in the *Transactions of the Edinburgh Obstetrical Society*, vol. xxxiv, pp. 303 to 357, and for additional anatomical knowledge as to free-martins the same worker gives much information in *Proceedings of the Royal Society, Edinburgh*, vol. xxx, pt. 3, pp 230-241.

Clinical and other Notes.

A CASE OF CONTINUED FEVER DUE TO BACILLUS ENTERITIDIS GÄRTNER COINCIDENT WITH A GUNSHOT WOUND OF THE CHEST.

By Major R. G. S. GREGG.
Royal Army Medical Corps, D.A.D. of P., 6th Division.
AND
Captain P. HAYES.
Royal Army Medical Corps.

No. 48489, Pte. "M," 1st Manchester Regiment, was admitted to the surgical division on August 19, 1920, on account of a gunshot wound of the chest received in an ambush a few hours previously.

The entrance wound was situated on the left side at the angle of the seventh rib, there was no exit wound. There was a slight hæmoptysis a few hours after admission. During the succeeding week the temperature continued between 100° F. and 102° F. and, as there was definite consolidation of the base of the left lung, the patient was transferred to the medical division on August 28, 1920.

August 28, 1920: Patient looked decidedly ill, anæmic and somnolent, complained of pain in the chest, respiration hurried and shallow, 40 per minute, cough troublesome, sputum scanty, purulent but not blood stained, pulse 120 per minute, of poor volume and a tendency to be dicrotic, tongue dirty, sordes on lips. Examination of the chest revealed consolidation of the left base, the abdomen was tympanitic, spleen markedly enlarged and could be felt two inches below the costal margin, the liver also was enlarged and palpable an inch beyond the corresponding costal margin. There was no paralysis nor loss of sensation in the lower extremities, reflexes both superficial and deep were normal. X-ray examination showed a fracture of the left seventh rib at about the site of the entrance wound, and a foreign body—metallic—lying on the spine of the seventh dorsal vertebra; no hæmothorax was present. Temperature was then 102° F. but that evening went up to 105° F.

August 30, 1920: Yesterday, a brisk diarrhœa developed, the stools contain some mucus and are very evil smelling. The temperature has continued high, and the patient's general condition is rather disquieting. He is placed on milk diet and stimulant and expectorants ordered.

September 1, 1920: It is now evident that the local lesion in the chest cannot account fully for the peculiar train of symptoms present and other causes are suspected. Blood is negative to malaria and the serum shows standard agglutination units to the following:—

B. typhosus	12
B. paratyphosus A..	30
B. paratyphosus B..	*Nil*

The inoculation history is somewhat ambiguous as only a temporary medical history sheet is available. The patient states he was inoculated, but he does not know whether it was against enterica or influenza. Temperature has a tendency to descend and the pulse and respiration rates are much improved. The stools are assuming a normal character and are negative to amœbæ and cysts.

September 3, 1920: Clinically the improvement noted in the last minute has been continued, the temperature reached normal yesterday morning. Cultural examination of the fæces shows B. Morgan No. 1 and no dysentery bacilli.

September 8, 1920: During the last three days the temperature has been making a somewhat step-ladder ascent and it is evident that a relapse has occurred, the dullness at the base of the left lung is more pronounced and there are signs of fluid being present. Total leucocytes 14,000 per cubic millimetre. Serum standard agglutinin units:—

B. typhosus	12
B. paratyphosus A	30
B. paratyphosus B	6·5

September 9, 1920: Total leucocyte count to-day is 15,300 per cubic millimetre and the relative count is as follows:—

Polynuclears	60 per cent
Lymphocytes	35 ,,
Large mononuclears	3 ,,
Transitionals	1 ,,
Eosinophiles	1 ,,

About two ounces of serum was drawn off on aspiration of the chest. Blood is negative to malaria and relapsing fever.

September 10, 1920: During the last three days the chart shows a marked "swing" in the range of temperature which has reached almost normal this morning. Patient is much easier, respiration quieter, yet the pulse rate is still over 100 per minute. Blood has been found negative again both to malaria and relapsing fever. The standard agglutinin units are as follows:—

B. typhosus	12
B. paratyphosus A	11
B. paratyphosus B	6·5

Fæces negative to amœbæ and cysts.

September 14, 1920: General improvement maintained. Cultural examination of fæces and urine shows, in the former, lactose fermenters absent, in the latter an organism in pure culture (called "X Cork") was obtained and has been plated out on litmus lactose agar.

September 18, 1920: Transparent blue colonies are present on the medium in pure culture. The organism is a Gram-negative motile bacillus and its macroscopic agglutination with high titre sera is thus:—

B. typhosus	Fine clumping
B. paratyphosus A	Negative
B. paratyphosus B	Very fine agglutination
B. dysenteriæ Shiga	Negative
B. dysenteriæ Flexner	Negative
B. aertrycke / *B. Gärtner*	Sera not obtainable

September 16, 1920: The same organism has been recovered again from the urine and is subcultured on agar slopes and put through sugars, etc.

September 17, 1920: The agar growth is confirmed morphologically. The sugar reactions (twenty-four hours) are:—

Lactose	Negative
Glucose	Acid and gas
Mannite	,, ,,
Dulcite	,, ,,
Litmus milk	Alkaline
Indol	Negative
Gelatine	Not available

September 18, 1920: The sugar reactions are unchanged after forty-eight hours. The patient's serum gives standard agglutination with laboratory organisms as follows:—

Using Dreyer's method:—

B. paratyphosus A	.. ..	1 in 40
B. paratyphosus B	.. ..	No agglutination
B. aertrycke Newport	.. ..	,, ,,
B. enteritidis Gärtner	.. ..	1 in 90
B. Morgan No. 1 ..	.. ..	No agglutination
B. "*X Cork*" ..	.. ..	<250 .. <500

The last organism ("X Cork") with high titre sera clumps as follows:—

B. typhosus (1,000)	.. ..	*Nil*
B. paratyphosus A (2,000) ..	..	*Nil*
B. paratyphosus B (2,000) ..	..	*Nil*

The laboratory report of this date is as follows· "Organism recovered from patient's urine is one of the Salmonella Group, probably *B. enteritidis* Gärtner, as it corresponds with this bacillus in its morphological and biochemical properties and is not agglutinated by high titre sera of *B. paratyphosus* B. and *B. aertrycke* Newport; it is further probably the causative organism of the patient's condition, as his serum clumps it in dilutions up to and above 1 in 250. The possibility of the patient's being a carrier of the organism cannot be excluded."

September 19, 1920: Patient is now convalescent. He has been questioned regarding his movements and diet previous to his being admitted to hospital, and states that he was then at an out-station, and that the meat supplied was frequently bad (probably maggotty), and that noticing this he never partook of it.

October 18, 1920: Progress since last note has been uneventful, transferred as a convalescent to Military Hospital, Queenstown. Before discharge from the above hospital the urine was proved to be sterile. Additional and confirmatory tests as to the nature of the organisms resulted as follows, reagents having by now been obtained.

Microscopic Agglutination.—High titre sera. "X Cork" with

Gärtner serum (1,000)	.. ..	Marked coarse clumping
Aertrycke serum ..	.. ..	Negative

By Dreyer's Method.—"X Cork" agglutinated Gärtner high titre serum (1,000) in all dilution up to "standard pulls" in 1 in 5,000. Gelatine stab no liquefaction up to ten days.

Absorption tests	End point of agglutination titre for *B. typhosus*, *B. enteritidis*, "X Cork"		
Gärtner high titre serum—			
Before absorption ..		1250	6250
Absorbed with Gärtner		<250	<250
Absorbed with "X Cork"		500*	<250
Patient's serum (December 1, 1920)—			
Before absorption ..	100	125	>500
Absorbed with *B. typhosus*	<25		500
Absorbed with Gärtner ..		<25	<25
Absorbed with "X Cork" ..	96	>25	<25

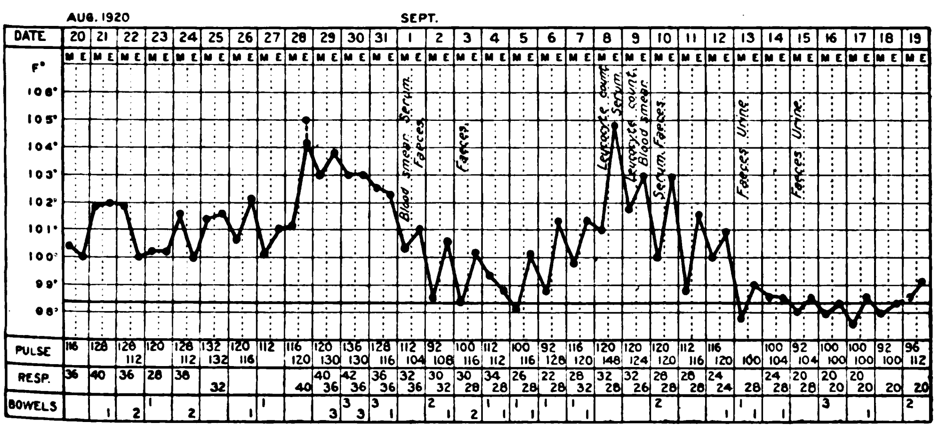
* "X Cork" unaccountably did not remove all the agglutinins for laboratory Gärtner from Gärtner high titre serum (tested twice); it succeeded in doing so from patient's serum; also it removed all agglutinins for Gärtner in tests performed in Lister Institute.

On November 30, 1920, the Curator of the National Collection of Type Cultures, Dr. R. St. John Brooks, who kindly investigated the organism in question, reported as follows: "'X Cork' is a typica *B. enteritidis* Gärtner.

Sugar reactions typical. Agglutination with 'Stokes' Gärtner serum 1600/6400. Absorption, 'X Cork' takes all the agglutination for 'Stokes' Gärtner out of Stokes serum. Investigation conducted by Colonel Harvey, I.M.S., in my laboratory."

Comment.—The diagnosis in our opinion lay between a condition arising from septic infection of the wound, and some generalized infection not connected with the wound and therefore coincident.

Against the first are the following facts: (1) No hæmothorax; (2) the type of the fever; (3) the enlargement of the spleen and liver; (4) the tympanites and at one time the diarrhœa; (5) absence of polynuclear leucocytosis, and (6) the isolation of the *B. enteritidis* Gärtner from the urine in pure culture on two separate occasions. A strong supposition therefore exists that it was a coincident infection and the next question to be decided is, what was its nature? The points in favour of *B. enteritidis* Gärtner are (*a*) recovery of the organism from the patient's urine; (*b*) the high agglutination titre of his serum. That the condition was due to an unknown cause and that the patient in addition was a carrier of



B. enteritidis Gärtner is possible but we think unlikely, as the high agglutination titre of the serum seems to favour an acute condition. However, it may be remarked that on December 1, 1920, this high agglutination titre showed no signs of subsiding for either the *B. enteritidis* Gärtner (laboratory) or the patient's own organism (" X Cork"), yet all examinations of urine subsequent to September 18, 1920, showed absence of this bacillus. Additional interest is lent to this case by a somewhat similar one recorded by A. B. Rosher, M.R.C.S., L.R.C.P.Lond., D.P.H., and G. Selbey Watson, M.D., M.R.C.P.Lond., D.P.H., in the *Lancet* of January 1, 1921, under the title of "A Case of Enteric Fever due to *B. enteritidis* Gärtner," in which the authors state that cases of this nature, running a typhoid course are unique and have not hitherto been published. From their patient the *B. enteritidis* Gärtner was not recovered, and the diagnosis was made primarily on the high agglutination titre of the serum. We agree with their diagnosis. We question whether the case should be designated enteric fever. We consider that Pte. M.—suffered from a condition due to the same cause, even though it might be asserted that the wound of the chest was partially or totally responsible for the pyrexia.

We wish to thank Lieutenant Colonel Dorgan, R.A.M.C., officer commanding Military Hospital, Cork, for his kind advice and criticism, and Colonel Beyts, C.B.E., A.M.S., A.D.M.S., 6th Division, for his permission to publish these notes.

NOTES ON A CASE OF ACROMEGALY IN A YOUNG SOLDIER.

By Captain C. F. BURTON.
Royal Army Medical Corps.

Bndsmn. A. J. W., aged 20. Service, five years three months. Transferred to convalescent depot, Montazah, Egypt, as nephritis, on September 22, 1920.

On Admission.—Patient complained of: (*a*) Constant dull headache chiefly confined to right frontal and parietal regions, with periodical exacerbations of acute pain referred to right side of neck—duration, two and a half years; (*b*) "puffiness" of face especially in the mornings—duration, four years; (*c*) aching pain and "tired feeling" in both calves—duration, six months.

Family History.—No cause to suspect hereditary abnormality of development. Father said to have died of tuberculosis.

Previous Medical History.—(Patient's own statement, no documents available to verify.) Winter cough up to the age of 15 years. Enlisted at the age of 15. Height on enlistment, 5 feet 3¾ inches. Date of enlistment, July 10, 1915. Christmas, 1916: Began to feel pain in calves and was told by room-mates that his face was swollen in the mornings. Remembers that his cap seemed to be tighter in the mornings. Did not report sick. Condition continued. June, 1917: Bad headaches, chiefly frontal, and dizziness commenced; reported sick. B.H.T. Winter, 1917: Difficulty in putting on boots in the early morning was noticed. All symptoms continued until September, 1918: Reported sick at Aldershot with headache. A specimen of urine is said to have been examined and found to have been normal. B.H.T. November, 1918: Reported sick again with same symptoms; urine again normal. About this time periodical swelling of feet became so marked that patient was unable to do physical training. September, 1919: Landed in Egypt. Headaches became more severe and constant. Reported sick twice. B.H.T. August, 1920: Admitted to hospital for circumcision; no operation. Ordered glasses by eye specialist whilst in hospital: glasses did not ease headaches. Transferred to convalescent depot, Montazah.

Condition on Transfer.—General appearance: Height, 6 feet 6 inches. Build: Very heavily built youth, well proportioned; head does not appear unusually enlarged; circumference of head at mastoid level, twenty-three inches; supra-orbital bosses marked. Skin: Coarse and rather shiny. Nose: Broad and flat. Ears: Not unduly thickened. Lips: Thickened. Marked puffiness under the eyes. Tongue: Normal. Jaw: Heavy. Teeth: Some separation between lower incisors and bicuspids. Hands: Finger tips extend to middle of thigh in upright position; broad, generally enlarged fingers, comparatively short, swollen and splayed; no clubbing. Feet: Comparatively short but broad; toes swollen and tend towards "hammer toe," great toes enlarged. Œdema: Slight, both legs, over lower third of tibiæ; not apparent elsewhere. Central nervous system: Patient appears depressed; memory and intelligence unimpaired; no spastic or paralytic condition; continual headache over right frontal areas and

Fig. 1.—Bndsmn. W. Acromegaly: Note supra-orbital bosses.

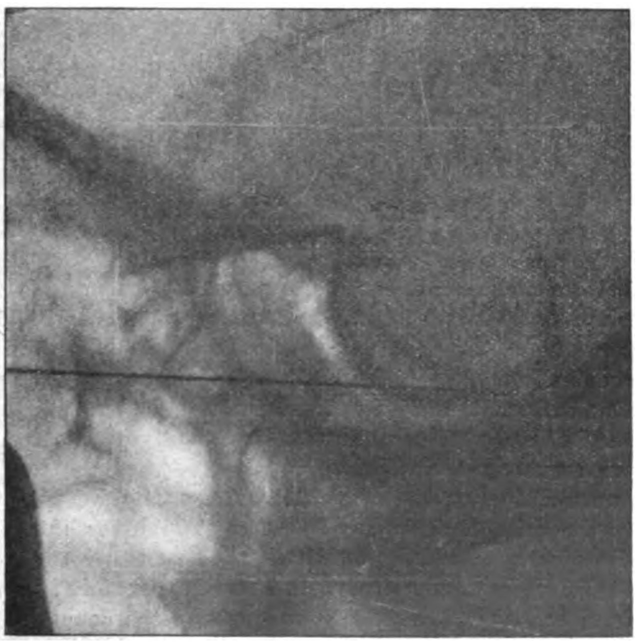


Fig. 2.—Bndsmn. W. Pituitary fossa: lateral view, showing enlargement and arching of post clinoid process.

right parietal areas. Reflexes: Normal, no inco-ordination. Cranial nerves: Of these, the trigeminal only appears to be involved. Eyes: Pupils equal, react to light and accommodation; movements normal. Eye specialist reports that there is no optic neuritis or atrophy and no abnormality except a slight redness of the disk. Circulatory and respiratory systems: Appear normal. Alimentary system: Appetite unimpaired, periodical dyspepsia, bowels act regularly. Urinary system: No history of specific disease. Urine: Reaction, acid; specific gravity, 1030; no albumin, blood casts or other abnormality found on three examinations.

X-ray Examinations.—(*a*) Head: Frontal sinus abnormally large; supra-orbital bosses due to this enlargement.

(*b*) Pituitary fossa: Enlarged; posterior clinoid process arching well over the dorsum sellæ (fig. 2). The fossa is enlarged to about two and a half times the normal size.

(*c*) Sphenoidal sinus: Less extensive than in normal case. Measurements of pituitary fossa by localization. Normal: Antero-posterior, $\frac{5}{16}$ of an inch; Bndsmn. A. J. W., $\frac{3}{4}$ of an inch.

(*d*) Hands: No mushrooming of bones of digits. General enlargement present, particularly of proximal phalanges.

Summary.—The case is of interest in comparison with that described by Captain Thompson, because of:—

(1) The comparative early age incidence.
(2) The insidious onset.
(3) The absence of ocular symptoms or signs.
(4) The undue enlargement of the frontal sinus.
(5) The superficial similarity to nephritis.

From a general impression of the case it appears that it is one of simple hypertrophy of the pituitary gland with involvement of the cranial nerve from lateral pressure.

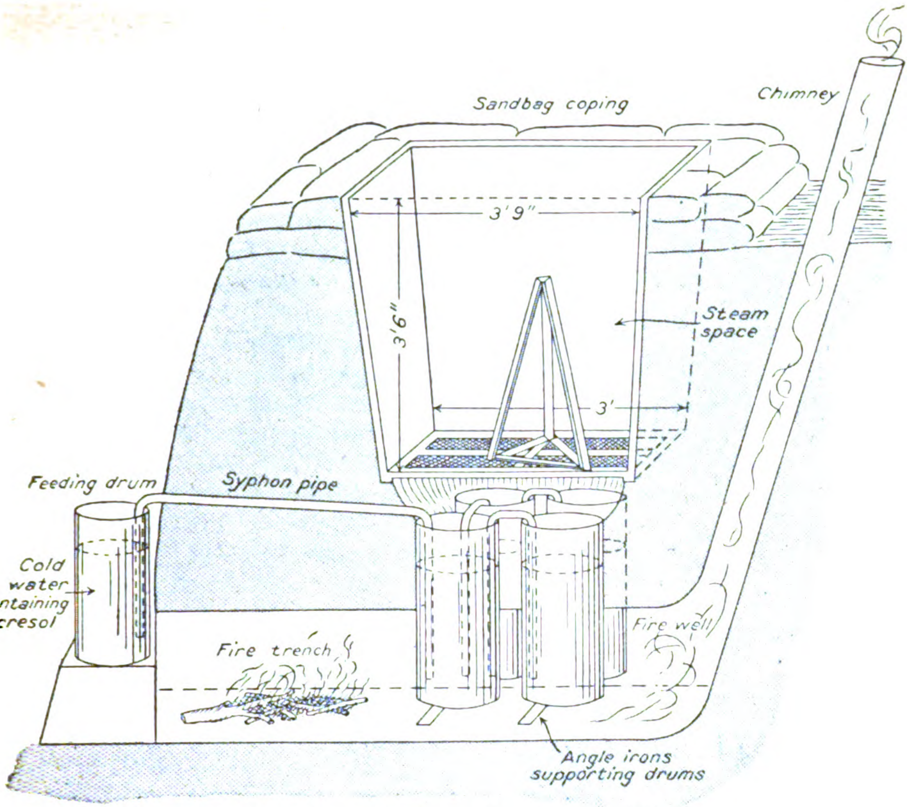
A DUGOUT STEAM DISINFECTOR.

By Lieutenant-Colonel P. H. Henderson.
Royal Army Medical Corps.

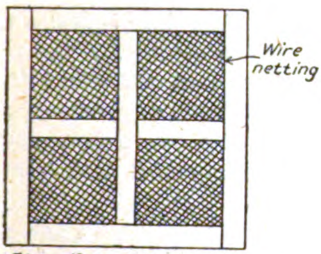
The disinfector depicted in the accompanying sketches was used with success in the Balkans by units of the 27th Division, during the years 1917-18.

It was originally devised to overcome the difficulties met with in transporting the Thresh, and Serbian barrels, which formed the authorized appliances for carrying out disinfection in the Division. Each battalion and unit of similar size was alloted four Serbian barrels, but, owing to lack of suitable roads and transport, these were frequently left behind.

The importance of keeping the troops free from vermin was impressed on all concerned, and the risks of spreading dysentery and other infectious diseases met with in Macedonia were fully realized. The necessity therefore arose of improvising a reliable disinfestor and which could be readily and quickly made by any unit from materials available in all parts of the country, and the contrivance depicted below was the result. The notes on the sketches explain the construction. The lid, which is omitted from the sketches, is made of wood

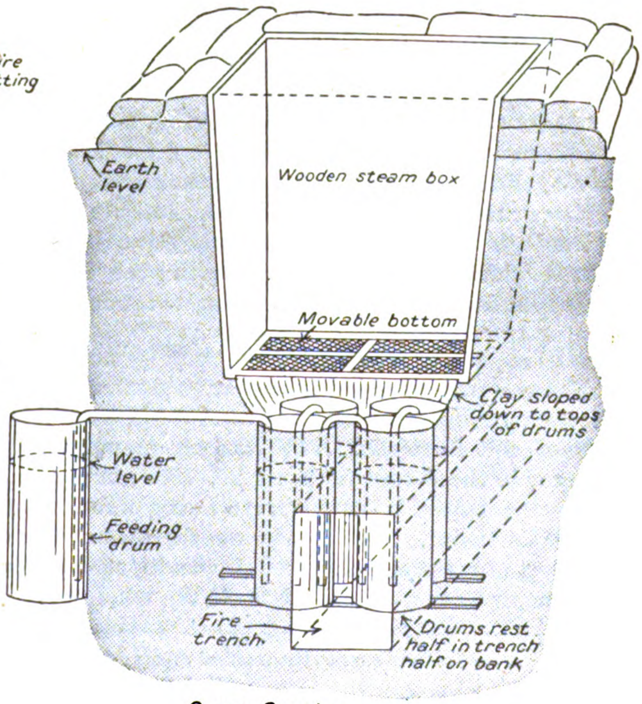


Section from Back to Front



Plan of movable bottom

Small triangular wooden frame for clothes, etc., to facilitate penetration of steam.



Cross Section

covered with old blankets and is weighted down with stones. Hooks and wires, on which to hang clothing, etc., are secured to the under surface.

Attention is particularly directed to the fifth kerosene oil drum which is situated outside the dugout and acts as the feeding drum for the other drums, by means of syphon action. This avoids the necessity of opening the steam box and so losing steam when adding water to the drums.

When a unit came out of the line, all the bedding and clothing was disinfected as a routine measure and in practice it was found easy to thoroughly disinfect the entire bedding and clothing of a platoon in one such disinfector in a day. Lice and nits were destroyed in twenty minutes, but forty minutes were usually allowed for the complete disinfection of any article. A disinfector of the size depicted could take twenty-five blankets or eighty shirts at one time.

Lecture.

ENCEPHALITIS LETHARGICA.[1]

By Major A. T. TODD, O.B.E.
Royal Army Medical Corps.
Consulting Physician to the Rhine Forces.

Early in the year 1918 an epidemic disease, characterized by unusual symptoms, appeared in the British Isles; these symptoms were so extraordinary that the attention of medical men and laity alike was engaged at once. When the frequently fatal issue and the complete failure of therapeutic measures became known, the interest in the disease, although increased, became less academic and more personal until it became evident that the contagiosity of the disease was very low, if, indeed, the disease could be regarded as contagious at all. Since then few diseases have received so much attention from medical and lay writers, and an enormous literature has sprung up, mostly dealing with one aspect of the disease, clinical, epidemiological or pathological, but to a large extent dealing with the few or many cases observed by the authors.

The occurrence of several cases of this disease in the Rhine Forces, and the interest which they have aroused among the medical officers, is the reason for this paper, in which I am attempting to condense some of the recent literature.

History.

The present epidemic apparently originated in Austria early in 1917, and was described by Economo; shortly afterwards cases were noted in France, then in England and Australia. The first accounts of the disease in British literature appeared in the *Lancet* of April 20, 1918; then the disease was thought to be botulism, a general infection due to eating of food containing the *Bacillus botulismus*, or its toxins. This theory was shortly disproved, and as no known pathological agent could be discovered, it was thought that a new disease had appeared and many names were coined to fit the condition. By this time, however, the

[1] A lecture delivered to the medical officers of the Rhine Forces, March, 1920.

disease is generally known to medical men of all countries as encephalitis lethargica or epidemic encephalitis, and time is too short to enter into discussion of the suitability of the various names proposed.

Presumably influenced by the saying, " there is nothing new under the sun," search was made in the medical writings of the past, and it became obvious that the disease was far from new. Some of the older practitioners remembered that after the influenza pandemic of 1890, there appeared in Northern Italy and Dalmatia a new disease which was then known as nona, and its possible advent into France was regarded with fear: this disease was characterized by fever, somnolence, tremors and various paralyses, and without doubt was the disease under consideration. A Commission was formed to investigate the conditions, but it did not reach the locality until all the cases had died or recovered [1].

In 1712 Camerarius described an epidemic with similar symptoms at Tubingen and called it Schlafkrankheit. Towards the end of the 16th century Sydenham describes a fever accompanied by somnolence, and lasting several weeks. About the same time Albrecht of Hildesheim describes a case of febrile lethargy resulting in strabismus of both eyes. From this period to that of the ancients is a gap in the history. Galen wrote of the lethargy, and noted the persistence of mentality despite the apparent comatose condition of the patient. Celsus has a chapter on lethargy, but certainly also applied the term to what we would now call the typhoid state. Hippocrates describes the fever, tremors and somnolence, and even pointed out a method of treatment at present employed, for he observed that when an abscess developed the patient frequently recovered. (*v.* Note A.)

Thus the disease has long been known, but how are we to fill the gap in the history? Centuries may elapse without notice of cases. Either the disease is one which from time to time breaks out *de novo*, or it is one of the pandemic diseases with a long and variable epidemic periodicity, such as plague or cholera. The former conception is hardly justifiable, and the latter needs qualification, for an endemic focus is known for plague and cholera, whereas none is known for encephalitis, and it is hardly likely that so extraordinary a disease could exist and not attract notice. It therefore follows that the virus must exist in a non-virulent phase as a saprophyte, just as the meningococcus does, and like it, from time to time, acquires virulence and becomes pathogenic. If this conception be correct we should expect to find cases with greater frequency in the inter-epidemic interval, such cases being of a milder type than that of the epidemics. As the literature is more closely searched many of these cases should appear, and already several have been reported—thus Kinnier Wilson describes one case in 1907, and Walsh another in 1913 [2]. A third case is described in 1875 [3].

Ætiology.

Much work has been done to discover the pathogenic agent, and many theories as to its nature have already been made and disproved, many of these will be considered under the heading of differential diagnosis, and will be briefly noticed at present.

At the outset the disease was thought to be botulism, and many other types of food poisoning were considered as possible factors—thus solanin poisoning, from changes occurring in badly stored potatoes; that it might be due to the

benzine employed in the manufacture of oleomargarine; [4] that it might be a deficiency disease, somewhat of the nature of beri-beri or pellagra, conditioned by a war diet; however, the disease also appeared in countries where food was abundant and of good quality, as in Queensland and the United States, and some of the theories were no longer tenable. A favourite theory for some time that the disease was merely an unusual type of Heine-Medin's disease, or poliomyelitis, was introduced by Crookshank [5] and supported by Osler and others. The possibility of the disease being of influenzal origin, especially in view of its appearance amongst the epidemics of influenza, was very soon considered, especially by the laity; this belief was strengthened when the epidemic of nona, after the 1890 influenza was remembered. That the disease is distinct from influenza cannot be doubted, for the symptomatology as well as the histological findings are quite distinct. But, though the exciting cause of encephalitis is not that of influenza, yet it is possible that the general reduction of the health of the community occasioned by the previous epidemics of influenza, was one of the predisposing causes.

As pathological investigation proceeded speculation diminished, for it was quickly realized that a definite infection was present. In 1917 von Wiesner investigated a series of Economo's cases, and claimed to have cultivated a Gram-positive diplococcus from the tissues of a monkey inoculated with brain from a case of encephalitis; this diplococcus reproduced the disease in other monkeys.

Other investigations failed to confirm this result [6].

In February, 1919, Bradford, Bashford, and Wilson described the presence of a filterable virus in encephalitis and some other diseases; they showed that the disease could be transmitted to monkeys by the injection of brain and other tissues. Also they claimed to be able to culture the virus by the Noguchi method [7].

Morse and Crump, 1920, isolated an organism which appeared to be a staphylococcus from six consecutive cases of the disease; it was culturable, and cultures injected into rabbits reproduced the disease; filtered cultures were also lethal, but this they regarded as being due to extracellular toxin [8].

McIntosh and Turnbull, in 1920, confirmed the presence of a filterable virus and transmitted the disease to monkeys [9].

Levaditi and Harvier conveyed the infection to animals by intraocular injection and by scarification of the nasal mucosa. They also showed that animals immune to poliomyelitis were not immune to encephalitis and vice versa [10].

Strauss, Loewe and Hirshfield have also cultured the virus by the Noguchi method, and demonstrated that it was a filter passer, also that though it is somewhat like the virus of poliomyelitis yet there are distinguishing features [11].

Maggiora, Mantovani and Tombolata are reported to have isolated from broth cultures of blood a small Gram-positive diplococcus, which was agglutinated by the serum of patients and convalescents up to dilutions of 1 in 100 [12].

It is thus seen that the investigations have led to some confusion, but some of this may be due to the employment of varying methods, for in the investigation of poliomyelitis Rosenow and Wheeler found a pleomorphic coccus in the brain and other tissues; this when cultured aerobically appeared as a diplococcus, but in anaerobic culture it became much smaller, was filterable and resembled the Flexner bodies noted in the tissues of poliomyelitis [13].

Quite recently de Fano [14] has described the presence of minute intracellular bodies in the nerve cells of the brain and the epithelial cells of the salivary glands. These appear to be minute coccoid bodies surrounded by a clear area somewhat resembling a capsule, but possibly due to hyaline degeneration of the protoplasm brought about by diffusion of toxin (Note B). We may therefore conclude that the disease is caused by a specific virus, probably filterable or with a filterable phase. Marinesco [15] is of opinion that this virus obtains entry into the body from the throat or nasopharynx, from whence it is conveyed to the brain via the lymphatics. As to the contagiosity of the disease there is very little real knowledge: though most of the recorded cases have been treated in the wards of general hospitals I have not heard of other patients in the same wards acquiring the disease. Netter says that contagion is rarely evident even where every opportunity for infection is present. He attributes this to the lethargy itself, and points out that a patient who is lethargic does not cough or talk, and it is the spray of infected mucus which accompanies these actions, which is the chief source of infection [16]. Flexner, however, suggests that it is the abortive and ambulant cases which spread the infection [17] (v. Note C).

Age and sex appear to have no influence on immunity or infectability; the sexes are equally affected and no age appears to be immune—infants, children, adults and the aged may all acquire the disease.

Seasonal Influence.

The disease is more frequent during the winter months, another difference from poliomyelitis, which is a summer disease.

Morbid Anatomy.

With the exception of the recent findings of de Fano there is general agreement as to the histological findings.

The central nervous system being excluded, there is little or nothing to be found peculiar to the disease: the changes in heart, lungs and other viscera, noted up to the present, are those usually met with in death from secondary infective processes—of which the most frequent are broncho-pneumonia and bed sores.

Brain may or may not show some apparent shrinkage with increase of fluid in the cisternæ. The dura mater shows no obvious changes: the pia-arachnoid is congested, and milky areas, due to infiltration of round cells, and small hæmorrhages, are frequently noted. On section, beyond congestion and the variable presence of small hæmorrhages, there is nothing noteworthy in the macroscopic examination. However, on microscopic examination definite changes are revealed and are most prominent in that part of the brain which produced the localizing symptoms: these changes are not present to the same degree in any other disease.

The lesions found are as follows:—

(i) Infiltration of the walls of the small arteries and veins with small round cells, chiefly lymphocytes and plasma cells: a few neutrophil cells and also eosinophil cells were noted by Marinesco and Draper—but McIntosh noted no neutrophil cells. The endothelium and fibroblasts may take part in the process. A hyaline degeneration (immediately external to the endothelium)

probably a later stage, was noted by Bashford and is present in the sections demonstrated [18].

(ii) There are also foci of interstitial inflammation—small accumulations of cells of a lymphoid type and occasional polymorphonuclear cells, and increase of the neuroglial cells.

(iii) The lesions of the nerve cells consist of dissolution of the Nissl bodies, more or less pronounced: reduction in size of the cellular body, and the number of processes with eccentricity of the nucleus, or even disappearance of the nucleus and vacuolation of the cytoplasm. The intracellular inclusions noted by de Fano are mentioned in the section dealing with ætiology.

(iv) Foci of hæmorrhage around the small vessels: the older hæmorrhages being represented by masses of fibrin and pigment.

Certain differences between the findings of encephalitis and other diseases may be dealt with here. In poliomyelitis the ganglion cells are much more markedly affected, many have disappeared and their places are occupied by phagocytes—whereas neuronophagia, though noted by Marinesco and McIntosh [19], is observed rarely in encephalitis. The perivascular infiltration consists of more polymorphonuclear cells and fewer plasma cells. The lesions of poliomyelitis are usually much more marked in the spinal cord. However, Mott, Marinesco and Draper are in agreement that the differences between the lesions of encephalitis and especially the ponto-bulbar form of poliomyelitis are more of degree than kind. In *botulism* there is no inflammatory reaction around the small vessels and no hæmorrhages—the lesion is more of the nature of a degeneration of the nerve cell itself. In influenzal encephalitis the perivascular infiltration is wanting, and the most marked change is the presence of hæmorrhages—presumably due to a lesion of the vascular endothelium.

SYMPTOMS.

The *incubation period* is not definitely known, but is probably variable like that of poliomyelitis. Experimentally, in monkeys, the incubation period was found to be eleven to thirteen days (Bashford) [20], and thirteen to forty-six days (McIntosh) [21]. The whole brain or any part of it may be invaded by the virus; moreover, in any affected nerve cell the virus or its toxins may lead to irritation, with increase of function and one set of symptoms, or cell death with cessation of function and a different set of symptoms. In any given centre for a definite nervous action, that is in any specialized nerve cell group, both sets of symptoms may be combined. It is thus seen that the symptomatology of encephalitis lethargica will not be simple, and the probability of finding even two cases exactly alike is very small—for the symptoms of any described nervous disease may be present. As a result many clinical types have been described, and names, chiefly indicative of the presenting symptom, have been applied to these so-called types, leading to some confusion, for it has been frequently observed that the type did not remain true throughout the illness. Walshe [22] therefore, advised that such a symptomatic classification should no longer be employed and suggested a classification on the lines which will be followed in this paper.

It is usual to recognize a prodromal period or stage of invasion, a stage of the declared disease and a stage of regression; these stages certainly exist and can be appreciated in any given case—but as there is no symptom of the stage of invasion

which may not also be present in the stage of the declared disease in a different case further attention will not be recorded here.

Symptoms may be discussed as follows :—

(1) General symptoms and signs probably due to the general infection.

(2) Nervous symptoms—due to local action of the virus on the central nervous system.

(1) *General Symptoms.*

Fever is usually present at some stage of the disease. It is irregular as to degree and type—usually irregularly remittent or intermittent; a constant type of pyrexia for a few days may occur. The fever is probably the first sign of the disease, and may occur some days before symptoms of a diagnostic character appear. The onset of a remission or a relapse is usually accompanied by return of the pyrexia. Shivering may accompany the fever. Later in the course of the disease a fever of the hectic type may develop, due to the presence of secondary infections.

The *pulse* rate is proportional to the height of the pyrexia—and is not increased or slowed in the apyretic periods. In the later stages of some cases rapidity or slowing may occur, and death from "asystole" is not uncommon. The presence of the normal pulse-temperature ratio is a point of some importance—for in tubercular meningitis characteristic alterations are found—undue rapidity and irregularity of the pulse.

Catarrhal Affections.—An early conjunctivitis is frequently present, and occurred in all the cases of any severity which I have seen. Later in the disease, conjunctivitis may reappear and is then probably due to loss of the blinking reflex. Slight pharyngitis—due to mouth breathing is frequently noted. Boveri [25] insists on the absence of bronchitis in the early stages, and points out that this differentiates the condition from the encephalitis of influenza, where an initial bronchitis is frequent.

Headache.—Whether or no the frequency is as great, at any rate complaint of headache is more frequently made in the early stages. It is at times one of the presenting symptoms. Where the lethargy is pronounced patients rarely complain of headache except to a leading question. General aches and pains are a feature of the early stages of the disease; more frequently still there is malaise.

Gastro-intestinal symptoms are frequently noted, but there is nothing characteristic of the disease noteworthy. Nausea, vomiting, and anorexia are often present, but these do not persist. Diarrhœa or constipation are frequently met with—often alternating. The mouth is generally dry and the tongue heavily coated, unless salivation is abnormally active.

Circulatory disturbances are noted with some frequency, but only in the later stage where there is local action on the cardiac centres or toxin action on the heart muscle itself.

(*To be continued.*)

Correspondence.

THE TACTICAL HANDLING OF FIELD AMBULANCES.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—The original letter from Major Keppel Read and the remarks of various other writers dealing with the above subject form a very interesting discussion, and one especially so to the writer, as it was with a division under his medical control that Major Read gathered some of his experiences.

The question as to whether the field ambulance should remain as a divisional unit under the command of the Assistant Director of Medical Services or become a brigade unit under the command of the Colonel-Commandant of the brigade has invariably led to diverse opinions.

Speaking as an Assistant Director of Medical Services of a division of the Egyptian Expeditionary Force operating in Palestine and Syria during 1918, under conditions both of defence and of very rapid advance, one can unhesitatingly say that, if any definite rule has to be laid down, the field ambulance should remain, as it is at present, a divisional unit.

Except when a division is actually engaged or advancing, there could surely be no doubt as to the field ambulance remaining under the orders of the Assistant Director of Medical Services. From a medical point of view it would be disastrous to contemplate any other alternative. The Assistant Director of Medical Services is the officer with the division, who above all is responsible for the welfare of the sick and wounded. It is his duty to be acquainted with the conditions and the medical arrangements of the lines of communications, and to know precisely in what manner the sick will have to be evacuated. No other than a medical officer could be expected to take the same interest in the perfecting of arrangements for the care and transference of the sick. How often has it happened that one has had to insist on certain arrangements being made, highly important from a medical standpoint, but which to the executive officer may have appeared altogether trifling.

It is only when one has to consider arrangements during a rapid advance that the real question of handing over the field ambulance to the Brigade Commander comes into play. It is possible, nay even likely, that when operating in difficult country, such as the Judæan hills, that the brigade and its attendant field ambulance may become completely cut off from the division. Such an eventuality can generally be foreseen, as was the case in Palestine in 1918, and arrangements should be made accordingly between brigade headquarters and the officer commanding field ambulance. For the time being, the officer commanding field ambulance will act under the orders of the brigade commander, until such time as direct communication can be resumed with divisional headquarters. It goes without saying that the Assistant Director of Medical Services must always be with his divisional headquarters or, if he should have to be absent for some special reason, the Deputy Assistant Director of Medical Services must be available to give information or advice to field ambulance commanders.

The allocation of field ambulances to particular brigades to remain and carry out all duties with them, but not to be looked upon as permanently belonging to them or under the orders of the brigade commanders, is an excellent one. It brings the officers and men of the units in contact with the personnel of the field ambulances and as they know each other better so are they more likely to work in harmony. It also enables the officers of the field ambulances to become well acquainted with the staffs of brigade headquarters and to understand each others' opinions and methods. Such intimate relations conduce to the smooth working of medical arrangements.

It must, however, be clearly understood by all concerned that should necessity arise, the field ambulance or part thereof, is liable to be detailed at any moment by the Assistant Director of Medical Services for other duty, outside the brigade, to which it may happen for the time being to be alloted. It should be incumbent and an act of courtesy on the part of the Assistant Director of Medical Services to notify the brigade headquarters before actually putting such an order in force.

With territorial divisions the brigades are apt to look upon certain field ambulances as part and parcel of the brigade, the personnel being recruited from the same localities. The idea is an excellent one, in so far as it leads to the wellbeing of the field ambulance being duly considered by brigade headquarters, but it must not give the latter the impression that the field ambulance is absolutely belonging to it or under its control.

I am, etc.,
C. E. P. Fowler,
Colonel, A.M.S. (R.P.).

May 18, 1921.

TREATMENT OF MALARIA.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

Sir,—I see injections of pancreatic amylopsin and trypsin have been used in malaria.

Will anyone in the tropics or elsewhere try injections of antivenin? In the intervals between acute attacks of malaria, it might be used in known malaria carriers and the effect, if any, on the parasite watched.

I am, etc.,
G. T. Rawnsley,
Colonel (R.P.).

Military Hospital,
Fort George, Guernsey.
May 9, 1921.

Notices.

EDITORIAL NOTICES.

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ASSISTED BY
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Original Communications.

MALARIA IN MACEDONIA, 1915-1919.

By C. M. WENYON, A. G. ANDERSON, K. McLAY, T. S. HELE,
AND J. WATERSTON.

Staff of Malaria Inquiry Laboratory, Salonika.

INTRODUCTORY REMARKS.

It is now general knowledge that the military operations in Macedonia were seriously handicapped by the epidemic of malaria which attacked the troops with such devastating results during the three years of our occupation. It would seem that practically every known means of combating this terrible disease was put into operation, yet in spite of all this expenditure of energy it is doubtful if any appreciable reduction in infections took place during our stay in the country, some parts of which cannot be described as anything but pestilential. In organizing the campaign against malaria it soon became evident that so many gaps occurred in our knowledge of the ætiology of the disease, its prevention and treatment, that a special Malaria Inquiry Laboratory was instituted, the duties of which were the investigation of any questions which might assist in eradicating or diminishing the incidence of the disease. The unit was recognized by the War Office as the Malaria Inquiry Laboratory, and was given the following War establishment:—

Detail	Personnel			
	Officers	Staff-Serjeants and Serjeants	Rank and file	Total
Lieutenant-Colonel or Major	1	..	..	1
Captains or Lieutenants	3	..	..	3 (*a*)
Laboratory Attendants	..	2 (*b*)	2 (*c*)	4
Total	4	2	2	8
Attached driver, A.S.C.M.T.	..	..	1	1
Total, including attached	4	2	3	9

(ii) Transport.

Detail	Vehicles	Drivers
Motor car	1	1 (*d*)

(*a*) 1 bacteriologist, 1 entomologist, 1 chemist.
(*b*) 1 staff-serjeant, 1 serjeant.
(*c*) Includes 1 corporal.
(*d*) Provided by the Army Service Corps.

Of this staff there was a Commanding Officer who regulated the work of the laboratory and undertook investigations on his own part, a pathologist, an entomologist and a bio-chemist, while a physician was attached to control the treatment of cases of malaria under special observation in the wards. The investigations covered a wide field, and the results have been collected in the form of a series of articles by the different workers, as follows:—

Part I.—"The Incidence and Ætiology of Malaria in Macedonia," by C. M. Wenyon, Temporary Colonel, Army Medical Service, Officer Commanding Malaria Inquiry Laboratory.

Part II.—"Clinical Observations on the Treatment of Malaria in Macedonia," by A. Greig Anderson, Temporary Major, Royal Army Medical Corps.

Part III.—"Hæmatological Investigations on Malaria in Macedonia," by Kenneth McLay, Temporary Captain, Royal Army Medical Corps.

Part IV.—"Chemical Investigations on the Excretion of Quinine by Soldiers in Macedonia," by T. S. Hele, Temporary Captain, Royal Army Medical Corps.

Part V.—"Entomological Observations on Mosquitoes in Macedonia," by J. Waterston, Captain, Royal Army Medical Corps (Territorial Force).

Every assistance was rendered by the Army authorities, and especially by the Director of Medical Services, Major-General Sir Maurice Holt, K.C.B., K.C.M.G., D.S.O., to whom the existence of the laboratory was, in the first place, due. The laboratory was fortunate in the competent staff it possessed, and had it been formed earlier, more important results would certainly have been obtained. The work was greatly facilitated by the energy and enthusiasm of Staff-Serjt. Weavis and Serjt. Muggleton, who rendered invaluable assistance.

PART I.

THE INCIDENCE AND ÆTIOLOGY OF MALARIA IN MACEDONIA.

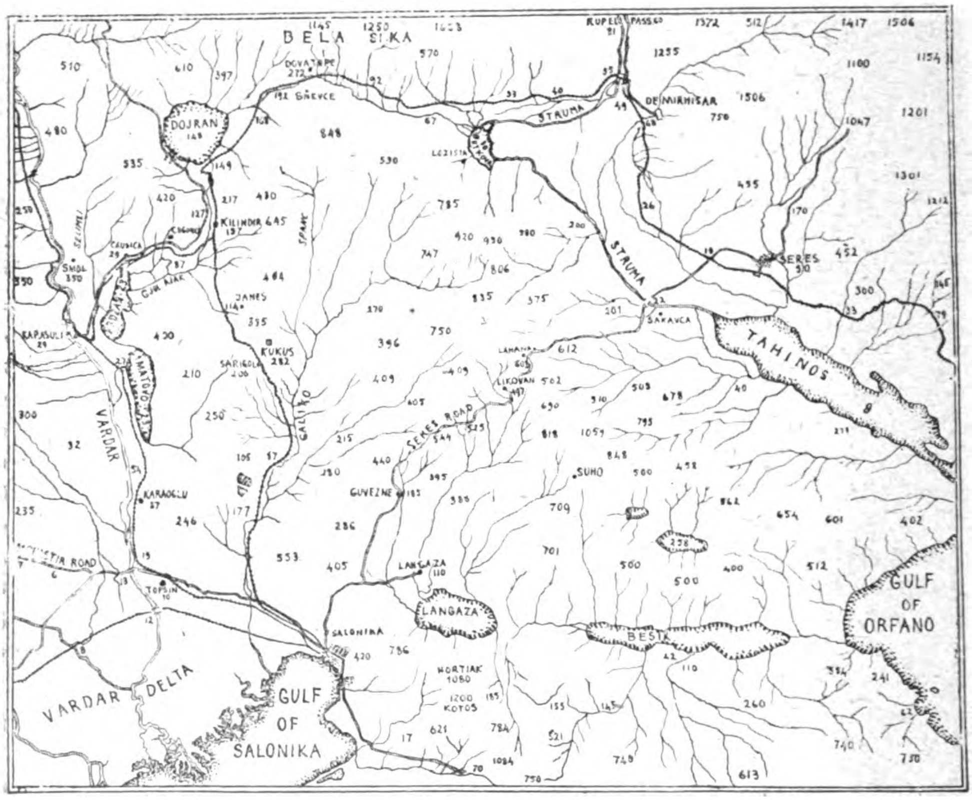
By C. M. WENYON.
Late Temp. Colonel Army Medical Service.
Wellcome Bureau of Scientific Research.

The history of the malaria epidemic in the Salonika army in Macedonia is now fairly well known, but a brief account of it will not be out of place here. It may be said to date from the movement of the troops forward at the end of June, 1916, to the low-lying valley of the Struma. The line finally occupied by our army extended in a curve from the mouth of the Struma river, along this as far as Lake Tachinos, and thence along the Struma river again to Lake Butkova, out of which the river bends suddenly northwards to the Rupel Pass, the gateway into Bulgaria. From Lake Butkova the line passed along the Butkova river and over the low watershed between this lake and Lake Doiran, and thence south of the lake in a south-westerly direction over the low hills as far as the Vardar river. To the immediate west of this the line was held by the French, and further west still by the Serbians and Italians. This long curve, about sixty miles in length, consisted of low-lying valley districts, and it partly enclosed a large area of hilly country which was broken in one place by the valley of the Langaza and Besik lakes and the Rendina river, a low stretch of country which extended as far as the Gulf of Orfano at Stavros, a short distance south of the mouth of the Struma itself. The line between Doiran Lake and the Vardar ran a little north of Smol, but the country south of this, between the Vardar and Lake Ardzan, was occupied by reserve troops and other units which extended over the country as far as Karasuli, the railhead of the supply railway from Salonika.

It will be seen therefore from the map that there is a circle of lakes connected more or less completely by rivers and bounded on the north by the high Bela Sika mountains, on the west by the mountains at the foot of which runs the Vardar river, and on the east again by more mountains which run down to the sea. The circle of lakes is completed by Lakes Langaza and Besik, while to the south of these and east of Salonika is more mountainous country, of which the peaks of Hortiak and Kotos are the most prominent.

To the west of Salonika and north and south of the Monastir road is the low-lying area of the Vardar delta. We see therefore that the circle of lakes is surrounded on all sides by high land, except at its south-west corner, where the Vardar loses itself in the extensive Vardar marshes. Furthermore, within this circle of lakes is enclosed an area which, as the map indicates, reaches a fairly high elevation.

Of the front line, described above, the section between the Gulf of Orfano and Dova Tepe ran mainly through the Struma valley, and was very low lying while the section between Doiran Lake and the Vardar was on a higher level and extended over low hills. The reserve troops of the Struma section were stationed on the foot hills or even higher up, while various hospitals, Royal Engineers, Motor Transport units and others were dotted all along the mountainous Seres road, that monument of engineering skill which formed the only channel of supply to the Struma



Map illustrating the area occupied by the British Army in Macedonia, to show the circle of lakes described in the text. The figures represent metres above sea level.

valley. The reserve troops for the Doiran area were on the hills about Gugunci and Kilindir and also on the Janes plain, through which runs the Gjol Ajak river. Similarly, the troops behind the Doiran-Vardar line were scattered along the hills between the Vardar and Ardzan lake as far as Karasuli, the railhead for Salonika. The valley of the Langaza and Besik lakes was not occupied to any extent, but the railhead of Guvezne on the rising ground to the north-east of the valley was the centre of supply for the Struma and was always occupied by large numbers of troops.

Looking at the elevations on the map it could be surmised at once that the whole of the circle of valleys and lakes with their connecting rivers was likely to be highly malarious, and this was unfortunately the case. The incidence of malaria on this line was in inverse ratio to the elevation, for, as already pointed out, the highest section of this line was between Doiran lake and the Vardar, and here there was less malaria amongst the troops than there was either east of Doiran lake along the Struma valley or south of Smol along the shores of Lake Ardzan. Karasuli was a notoriously unhealthy spot,[1] as also were Causica, Lozista near Butkova lake and the swampy area at the mouth of the Struma.[2] These places are merely mentioned by way of illustration, for every spot along this line was heavily infested with anopheline mosquitoes. It may be stated here, though the matter will be referred to in greater detail below, that *A. maculipennis* was the prevalent valley mosquito, while *A. superpictus* was essentially a hill stream species, and was found all over the high land.

To return now to the movement of the troops forward in June, 1916, it may be stated that this was undertaken from strategic considerations with which we have nothing to do. The line occupied before this was on the hills south of Lakes Langaza and Besik and along the low hills east of the Galiko river as far as Kukus. Troops, of course, also occupied Salonika as base, and were scattered west of the town on the Monastir road. Apart from the latter area, this was not a very highly malarious country —though, as we shall see later, the mountainous districts also supplied no small fraction of the total of malaria-infected individuals. For the first five months of the year 1916 there had been only fifty cases of malaria. As the troops had not yet passed a summer in the country, and as the malaria season had hardly started then, some of these cases must have been relapses of a previous infection contracted elsewhere. In June there were 90 cases, and of these 36 came from the Monastir road area, while

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[1] The railway supply officer at Karasuli in 1917 was in charge of a unit of sixty men. Between July 6 and 25 thirty-nine men went down with fever. In practically every tent examined in this area on July 26 were many *Anopheles maculipennis* gorged with blood.

[2] Between Lakes Butkova and Doiran is a low watershed, and at this point was a post on a low spur known as Dova Tepe. Mosquitoes travelling up from the swamps below and down from the hill streams infested this spot. The spur was under shell fire, and little anti-mosquito work could be done, and if it could have been it would probably have been useless on account of the extent of the marshes in the valley and the hill streams above. On June 14, 1917, the 5th Connaught Rangers occupied this place for one week and were afterwards stationed higher up with only posts on the spur. They had no nets till the end of June. On July 16 they were relieved by the 1st Leinsters, who were provided with mosquito nets, veils and gloves, and mosquito-proof shelters were used by the men on all the lower spurs. The total amount of sickness of these two units during the eighteen days, commencing at the beginning of the third week after taking up the position, was 271 for the former and 123 for the latter. The better protection of the Leinsters probably accounts for the lower figure, for they should have had more malaria in July and August than the Connaught Rangers had in June.

the remaining 54 were from the Divisions. There was thus a definite increase in June, and it is probable that had the troops remained where they were, there would have been a much greater rise in the number of cases during July, August, September, and October — the worst malaria months in Macedonia—though the figures would certainly never have reached that appalling magnitude which resulted from the forward advance at the end of June. Even the high plateau of Hortiak east of Salonika ultimately turned out to be malarious—and, indeed, this was true of all the hill country of Macedonia.

At the end of June, as stated, the troops were moved forward more or less to the line indicated above. There was practically no provision made for protection against mosquitoes, but there was some attempt to protect against infection by the administration of quinine, six grains per diem. The Division which occupied the Doiran-Butkova section took up this line at the end of June, during which month there had been only four cases of malaria. Before entering the line the troops were given quinine daily, but its administration cannot have been carried out very systematically. Within twelve days of their arrival cases of malaria occurred and the admissions quickly mounted to over 100 a day, giving a total of 1,861 for the month of July. One Battalion (2nd East Surreys) entered the valley approximately 800 strong and left with 200 when the Division moved to the high land behind after a stay of six to seven weeks. The march back of half a day caused a further eighty to fall out sick. For the whole Division during the seven weeks there were 4,700 cases of malaria—though four weeks previously as many troops as could be spared had been moved to the hills behind. The history of the other units of the Division was a similar one.[1] At the end of August the Division again occupied the valley, and the cases of malaria rose at once, giving the following monthly figures:—

June	4
July	1,300
August	2,500
September	1,600
October	1,100

At the end of August, when the original Division again entered the valley, two other Divisions took up positions in the line east of this one. They had a similar experience, so that the troops in the section from Doiran to the sea contributed over 15,000 cases of malaria to the sick list of the Army. The troops of the two Divisions in the Doiran-Vardar section were not so heavily infected as those in the Struma valley, though they contributed well over 8,000 cases up to the end of 1916.

[1] During the seven weeks the 2nd Cheshires had 780 cases, the 3rd Middlesex 354, the 2nd East Yorks 320, the 2nd East Kents 360, and the 2nd East Surreys, just mentioned, 514.

On the lines of communication there were over 4,000 cases, which in itself is a proof that, quite apart from the Struma valley and the Doiran-Ardzan areas, malaria was very widely spread throughout the whole of the country.

During this year, 1916, large numbers of infected troops were evacuated to Malta and the United Kingdom, where they continued to have relapses. In all there were over 30,000 cases of malaria in 1916. These unhappy experiences caused the military authorities to think seriously about the following year and all the well-known methods of malaria prevention were undertaken. When the troops first occupied the front line the villages dotted along the fertile valleys were still occupied by their native inhabitants, who themselves were ridden with malaria. They undoubtedly constituted the foci from which our troops were infected in the first place. The villages were then evacuated but there were so many infected individuals and carriers amongst our own men by this time that there was ample material for mosquito infection without the necessary intervention of the native. It was shown by Captains Adams, Wilkin, Hele and Treadgold, who conducted investigations under the direction of Colonel Dudgeon during the winter of 1916-17, that a very high percentage of men who had previously had malaria still harboured parasites in their blood. Thus of 977 men of one series examined, 216 showed *Plasmodium vivax*, 24 *P. falciparum*, and 1 *P. malariæ*. A later series of 828 men gave *P. vivax* in 222, *P. falciparum* in 9, and *P. malariæ* in 1, while a third series of 1,031 men gave *P. vivax* 251, *P. falciparum* 0, and *P. malariæ* 0. These figures not only show the high percentage of carriers amongst the troops in the line but illustrate a point to be mentioned later, namely, the tendency for the *P. vivax* infections to persist during the winter while the *P. falciparum* infections tend to disappear spontaneously or as a result of quinine.

Arrangements were made for the drainage of swamps and clearing of streams, larvæ were killed by oiling and other methods, mosquitoes were destroyed in buildings and dug-outs by fumigation and spraying, mosquito nets were introduced on a large scale (there had been an almost complete absence of these in 1916), mosquito-proofing of huts and dug-outs was commenced, the men were taught the dangers of the mosquito, the methods of avoiding infection, the use of the protective appliances supplied to them—mosquito nets, head nets, gloves, shorts which could be turned down, repellents, and the reason for the administration of prophylactic quinine. It was still necessary to occupy the line of the previous year and it was arranged that a minimum of troops was to stay on the low ground, and as many as possible were to camp on the high land enclosed by the circle of lakes already mentioned.

It is evident to anyone who comprehends the problem of anti-mosquito work that to completely exterminate the mosquito from the whole of this vast area was an impossible task. Under peace conditions it would require

thousands of labourers whose sole duty was this particular work, elaborate schemes of drainage would have to be undertaken and millions of pounds spent on carrying this out. Even then it is doubtful if anything very satisfactory could be accomplished in less than five or more years of continuous work. Our troops, however, were not under peace conditions and they not only had to conduct a war but had to fight the mosquito as well. The whole Struma valley was the front line and it, with the streams, rivers and lakes, formed vast marshes which were under the fire and observation of the enemy. Much of the drainage and clearing work could only be carried on at night and even if this were done, beyond the line there was enough untouched breeding ground to supply sufficient mosquitoes to infect the whole Army. Though a great amount of very careful work was accomplished in the front line area it was evident that the men could only be satisfactorily protected by the contrivances used to prevent their being bitten by mosquitoes. On the other hand the hill country enclosed by the front line on which the troops not required in the valley were encamped, the district between the Vardar river and lakes Ardzan and Amatova, the large plain of Janes through which the Gjol Ajak runs south of Doiran, and the whole of the base area, including the marshy tracts of the Vardar Delta, the valley of Lakes Langaza and Besik, and the high land south of this together with the Salonika area itself, were not subject to the interferences of the enemy and could be dealt with according as labour was available. In these areas behind the line it was evident that there was more prospect of carrying out uninterrupted work in such a way as to bring about a definite mosquito reduction. The work, to be effective, had to be on a vast scale and had to be done by the troops themselves or local labour hired for the purpose. The whole occupied country was divided into areas, which were under the supervision of anti-malaria officers, and every unit had to contribute labour. The work was commenced early in 1917 and extended through the summer under great difficulties. It was recommenced in 1918 on a much larger scale and with additional native labour in the form of anti-malaria squads, so that the amount of anti-mosquito work in this year was at least ten times as extensive as that carried out in 1917.[1]

[1] The extent of the work and the amount of labour involved is well illustrated by the following estimates for 1917-18. In one Divisional area 47,306 yards of canalization were done and 37,106 square yards of ground cleared in one month. In the base and line of communication area: new trenches cut, 479,991 yards; old trenches renewed, 1,670,506 yards; number of pools drained and filled in, 9,690: water surface oiled, 1,157,192 square yards; brush-wood cut, 363,315 square yards. Very roughly the line from the Gulf of Orfana to Lake Doiran was sixty miles in length and this extended continuously through low-lying *A. maculipennis* breeding ground which varied in width from five to ten miles, giving an area of about 450 square miles. The hill country behind this line, all of it potential breeding ground for *A. superpictus*, is, if estimated on the flat map, at least three times as great. It resulted therefore that anti-mosquito work, except in certain localities, could only be done in patches and in most cases the good effect was lost owing to the untreated areas around these.

At the end of the first malaria season of 1916 the worst cases of the disease had been removed to Malta or the United Kingdom but, as we have seen, there were large numbers of carriers left behind amongst the men, especially in the Divisions. These carrier cases continued to relapse

ADMISSIONS FOR MALARIA, 1916, 1917, 1918.

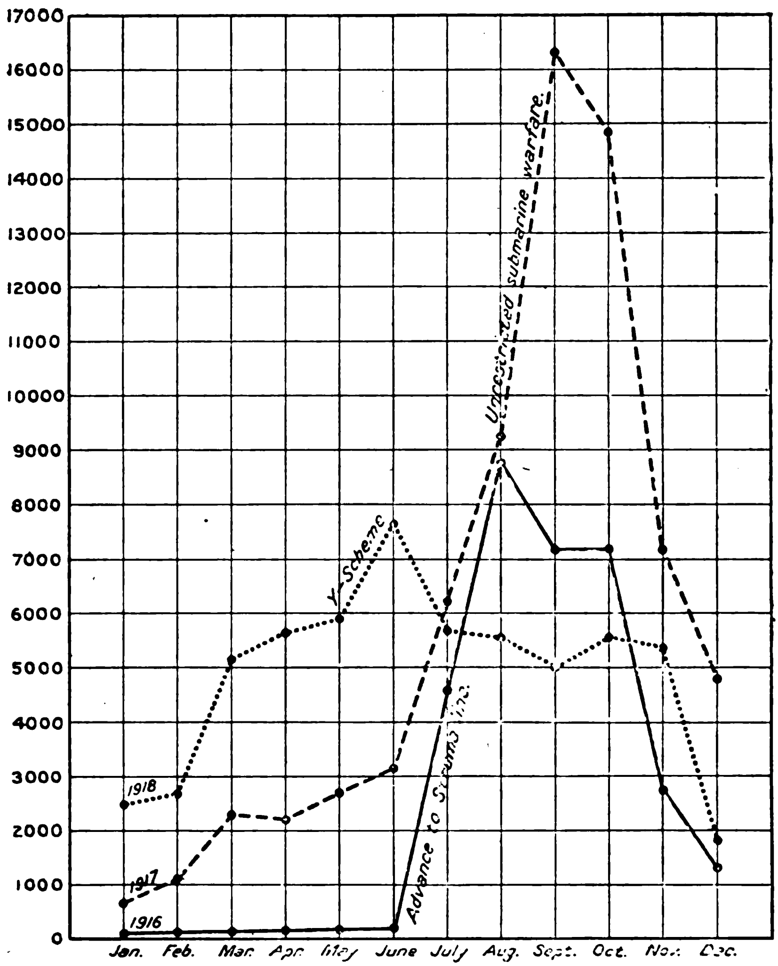

CHART I.

and formed admissions to hospitals during the first five months of the year. There were about 150 admissions for the first week in January and these increased fairly regularly till the middle of March when there were about 600 admissions a week. They remained fairly constant till the middle of June when there was a marked increase.

Though there had undoubtedly been many primary cases of infection before this, during May and June and possibly the latter part of April, the bulk of these admissions were relapse cases. Some of the supposed primary cases were really first attacks of malaria in men who had been infected months before. Such latent infections were later recognized as of quite common occurrence. From the middle of June onwards the numbers rose rapidly till they stood at about 4,500 for the first week in September. There was then a steady fall to over 600 for the last week in December. This enormous figure of over 70,000 for the year 1917 can be partially explained by the occurrence of the unrestricted submarine warfare and the impossibility of sending the sick away. Cases which otherwise would have been evacuated had to be kept and they returned to their units and came back again to hospital, some of them many times in the course of the year, so that a single case might account for many admissions. There is no record, as far as I am aware, of the actual number of infected individuals in the Army. During the year the number of troops in the Army was reduced by the removal of a Division and early in 1918 there was a further reduction by another Division.[1]

The history of the year 1918 is a similar one, except that the number of already infected individuals was very much higher than at the beginning of 1917, as evidenced by the increased number of winter cases of malaria. Thus for the first week in January, 1917, there were 150 admissions to hospital of relapsing cases, for the first week of January, 1918, there were about 650. The figure then rose again till in the middle of March there were about 1,500 admissions per week and this level was maintained with slight elevation up to about 1,700 till the time of the rapid rise at the middle of June of the previous year. It was realized that the Army was saturated with malaria and that unless something drastic was done the sick rate and mortality would be higher than it was in 1917. Accordingly the famous Y-scheme was devised whereby a careful inspection of all the troops in and out of hospital was made, their malaria history and general physical condition being considered with a view to weeding out the badly

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[1] In the period July to October, 1916, the admission rate for primary malaria per 1,000 of strength was calculated as 219, while for the same period in 1917 it was 256. The total number of admissions to hospitals and casualty clearing stations for the period January 1, to October 31, 1917, was 60,199 or 305·3 per 1,000 strength. In 1916 the corresponding figures were 28,254 and 242·7. The number of days' service lost through malaria was well over one million in 1917 and close upon two million in 1918.

The admissions for malaria per 1,000 of strength for the period November 1, 1915, to October 31, 1918, were as follows :—

November 1, 1915, to April 30, 1916 ..	0·24	May 1, 1916, to October 31, 1916 ..	237·28
" 1, 1916, " " 30, 1917 ..	56·83	" 1, 1917, " " 31, 1917 ..	277·85
" 1, 1917, " " 30, 1918 ..	162·75	" 1, 1918, " " 31, 1918 ..	253·82

These figures represent only a part of the truth. Thousands of cases of malaria were treated in field ambulances or in the men's own units without being admitted to hospitals, to which the above figures refer.

infected individuals who were of little service to the Army. These men were evacuated by the newly opened up overland route via Itea and Taranto. As will be seen, this scheme had a very marked effect on the malaria admissions. Instead of the figures rising as they did the previous year of 1917 they fell, and varied between 1,000 and 1,600 per week till the end of October, when there was a gradual but steady decline to a little over 500 for the last week of December. The Y-scheme not only reduced the number of cases by the removal of the most heavily infected individuals and so undoubtedly prevented each of these men coming into hospital several times but also got rid of the worst carriers, who would have been responsible for the infection of vast numbers of mosquitoes. In all, over 25,000 men were sent away and it can hardly be an exaggeration to suppose that during the summer and autumn these men, if they had remained in Macedonia, would have contributed fifty or sixty thousand admissions to hospital. Had this been the case the total number of entries for the year 1918 would have been considerably higher than in 1917. As it was, from the time that the great summer rise had occurred in 1917 and might have been expected to occur in 1918 there was actually a fall in the admissions and no definite rise occurred. It has been claimed by some that this failure of the summer rise to appear was a proof of the efficiency of the anti-mosquito measures adopted. It seems to me that there is no evidence of this whatever. Mosquitoes unfortunately still remained in many places where most energetic work had been carried out and there can be no room for doubt that the relative improvement was due practically entirely to the removal of the most debilitated cases.

The malaria incidence of 1918 was affected very seriously by the outbreak of influenza in September. Some confusion in diagnosis inevitably occurred, for so many men had latent malaria which was lighted up by the influenza that it was in some cases difficult to decide which was the primary disease. Thus of over 100 autopsies performed by Captain Taylor on cases which had been diagnosed as influenzal pneumonia eighty-three per cent showed definite evidence of previous malarial infection. It was only in a very small percentage of these that the diagnosis was found to be in error and that active malaria, and not influenzal pneumonia, had been the direct cause of death. This figure of eighty-three per cent shows furthermore to what a large extent the army of Macedonia had become infected with malaria.

Following the influenza outbreak, and partially concurrent with it, was the great advance into Bulgaria at the end of September. The figures for malaria show a rise from about 1,100 to nearly 1,600 per week to the beginning of October and then a fall to the end of the year.

The foregoing is a brief outline of the history of malaria in the Salonika army for 1916, 1917 and 1918. No one was prepared for the great epidemic of 1916 in the Struma valley and practically no precautions were taken there—it is doubtful if they could have been. In 1917 the whole anti-

malaria campaign was organized and placed on a working basis while in 1918 it was improved and practically every conceivable method was in operation, so that the work reached as high a level of perfection as seemed possible for an army in time of war in a mosquito-infected country like Macedonia.

The possibility of an outbreak of malaria in the area occupied by the Army before the June advance in 1916 to the Struma valley had been recognized by the authorities. Surveys had been made, spleen indices of the natives in the Langaza valley had been taken, and a good deal of drainage work, especially along the Monastir road, was in progress. Unfortunately an arbitrary limit had to be fixed to any work here, so that the good results which might have been expected were spoilt by the vast extent of breeding-ground immediately beyond.

To estimate the relative effect of the various anti-malaria measures which were adopted is a very difficult matter. As regards those directed against the mosquito breeding places the only test of efficacy is the reduction in the number of mosquitoes. It is undoubtedly true that the destruction of a single breeding place and the larvæ in it will reduce the number of mosquitoes, but when we realize that along the front line or the circle of lakes referred to above only a very small fraction of the total breeding ground within a mile of the troops could be dealt with we can understand that though the number of mosquitoes was slightly reduced this could have little effect on the incidence of malaria. The chances of infection will not increase in direct proportion to the number of mosquitoes after a certain limit. When the number of mosquitoes attacking an individual amounts to hundreds, if not thousands, the reduction of these by a small proportion will hardly lessen the man's chances of infection. It matters little whether he is bitten by ten or only five infected mosquitoes in a single night. It would thus appear that the clearing measures undertaken in the long line of swamp regions in the valleys can have done very little to reduce the number of mosquitoes and practically nothing to lessen the incidence of infection. Of the front line the Doiran-Vardar section was an exception to the general rule that the line was in low-lying swampy valleys. Except to the south of Lake Doiran where there was a good deal of marsh,[1] but not nearly as extensive

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[1] Along the south shore of Lake Doiran was an extensive marsh nearly a mile in length. It was covered by trees and thick brush-wood with swamps and tall grass interspersed. The strip seemed to be below the level of the lake and had apparently resulted from the digging out and piling up of earth to form the railway embankment years before. The positions on the low hills overlooking the lake were infested with mosquitoes from the marsh. In 1917 the position here was occupied as follows :—

12th Lancashire Fusiliers	June 10 to 26
14th King's Liverpool	,, 26 ,, July 12
7th South Wales Borderers..	July 13

During June there were practically no mosquito nets but this was rectified at the end of

as around the other lakes, the line passed over the low hills between the lake and the Vardar river. In this section the conditions were very much like those which obtained on the high land within the circle. Instead

Fig. 1.—Barren type of country in the gullies of which, however, were numerous streams harbouring *A. superpictus.*

Fig. 2.—Hill country with bushes and trees and numerous streams harbouring *A. superpictus.*

of the extensive swamps formed by the lakes and rivers overflowing their banks in the low-lying valleys the country was a system of hills and gullies. The hills, except in certain places such as Stavros, were devoid of forest, so

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June and during July great care was taken in the use of nets, veils, gloves, etc. Quinine was given, ten grains twice weekly, while the troops nearest the lake had ten grains every day. The following were the sick admissions to field ambulances for eighteen days commencing from the beginning of the third week (two weeks allowed for incubation period of malaria): Lancashire Fusiliers 252, King's Liverpool 156, South Wales Borderers 58. It might have been anticipated that the figures would rise in July and August but the better protection had made the difference.

that rain was not held up. They were green in the spring and early summer but the grass was quickly withered by the sun and the general aspect became that of a brown, dried-up country. The small valleys and gullies between the hills remained green and often contained trees and brush-wood which concealed the water running beneath them. In every gully there was a stream which was a rapid torrent in the winter or during

FIG. 3.—Gully with grass-covered stream in which *A. superpictus* commonly occurred.

FIG. 4.—Likovan stream, near Lahanah, in which *A. superpictus*, *A. maculipennis*, *Culex hortensis*, *C. mimeticus* and other mosquitoes occurred.

sudden storms of summer but which soon became a mere trickle, forming rocky pools green with algæ, or little reedy marshes, according to the nature of the ground. A typical hill stream in Macedonia commenced as a spring high up near the summit of some mountain two or three thousand feet above the sea. Such a spring was often converted into a fountain by the natives as a water supply for some neighbouring village or for the

convenience of shepherds or other wanderers. The overflow from the spring or fountain, the built-in part of which frequently harboured mosquito larvæ, especially *A. bifurcatus,* as well as frogs and newts, formed the source of the stream. Around the fountain there was generally an overgrown marshy patch of ground in which larvæ and frogs abounded. The stream trickled away between the rocks, forming the numerous tiny grass-grown pools in which larvæ thrived. It was quickly joined by other tiny streams and gradually became larger. It would flow across a comparatively level stretch of grass forming marshy patches of various sizes or

FIG. 5.—Rocky pool harbouring *A. superpictus*, frogs, snakes, water-boatmen and other creatures.

FIG. 6.—A hill stream crossing the barbed wire near Lake Doiran, in process of being cleared.

little reed-bordered ponds swarming with frogs, snakes, water boatmen and culex and anopheles larvæ. Further on it would pass through dense brush and be followed only with the greatest difficulty. Here in the deep shade anopheles larvæ and other living creatures were not as plentiful as in the open reaches. Lower down, the stream would break in a small cataract in stony ground or fall over a ledge into some deep rocky pool. In the shade

the rocks were overgrown with moss and ferns and the pool would harbour the ubiquitous frog, many species of which abound everywhere, snakes and even fish, all of which lived in harmony with the ever-present mosquito larvæ. Below the pool the stream would start on its course again, always joined by others on either side. On sandy or gravelly soil it would suddenly disappear completely below the ground and only be traced again a hundred yards or more lower down the gully. In the interval pools and marshy patches might still indicate its presence. Finally it would become large enough to receive such a name as the Gumus dere, Orljak dere, Copachi dere, regular little rivers which broke from the hills on to the plain. In the winter-time and in the spring and early summer these rivers continued their course across the valley into the Struma river, not omitting, however, to spread out over the plain into extensive marshes. As the summer advanced the hills dried up and less and less water descended. The streams were reduced to tiny trickles and when the valley was reached the water disappeared underground and only reappeared in the plain in the swamps and marshes, which were helped in their formation, in many places, by the level of the valley being below that of the river itself. High up in the hills in the summer-time water still appeared at the source and the endless ramifications of the tiny stream system still persisted, with its small pools and marshes always harbouring frogs and mosquito larvæ, even as high up as 4,000 feet above the sea.

It can well be imagined that to deal properly with such an endless system of streams would entail a stupendous expenditure of labour. In times of peace the country is only used as a grazing ground for sheep and goats. The rocky nature of the soil, except in patches here and there, renders it useless for other purposes so that any expenditure of labour from an anti-malaria point of view is hardly worth considering. It was only when occupied by an army liable to infection that the question presented itself.

In coping with such an extent of streams it was first necessary to obtain an accurate chart of the whole system, including all the little tributaries, many of which were often difficult to discover and trace. Brushwood and grass had to be cleared away, a clean cut channel had to be formed by digging, blasting, removal of rocks and boulders, and by opening up pools; water had to be siphoned off, areas of depression filled in, the edges of the stream lined with stones—and all this had to be done for every stream and its tributaries right up to the source of each. When the work was completed it had to be constantly watched and repeated, the channel brushed out and the edges oiled or cresoled, for the work was constantly damaged by horses, cattle, sheep and men, but most of all by the sudden downpours of rain which occur in the summer months. A tiny stream would be converted in a few minutes into a roaring torrent, undoing in an hour the work of many weeks. The summer downpours had one advantage. All the streams were suddenly flushed out and millions of larvæ

were washed away and destroyed. It was always very difficult to find mosquito larvæ in the streams for a week or two after a thunderstorm.[1]

It was in country of this nature that camps were placed, but it was only possible to deal with the streams in a limited radius round each. With sufficient labour it would be no impossible task to keep an area like this free from mosquitoes, but unless the work extended for very much farther than half a mile mosquitoes would wander into the area from beyond. Mosquitoes in Macedonia will travel a mile or more—very much farther than the limit of range formerly attributed to them.

Stream work of the kind described above was carried out over the whole of the high land occupied by the troops in addition to that just referred to in the Doiran-Vardar section, in fact over the whole country directly south of the Struma valley and on either side of the Seres road, on the hills to the west of Lakes Ardzan and Amatova, and on the large Hortiak plateau, the great summer hospital area south of Lakes Langaza and Besik..

The plain of Janes with the Gjol Ajak river afforded a district of a somewhat different kind, as also did the plain of Snevce south of the mountains between Doiran and Butkova. The former was an extensive tract of country reaching from Janes across to the shores of Lake Ardzan and northwards as far as Gugunci and Kilindir, where it merged into the hills (see map). This plain was cut into by the Gjol Ajak and numerous streams which, on account of the flat country, were often invisible till approached. In certain places only pools occurred and numerous marshes existed which were much more extensive and unmanageable near the reed-bordered Ardzan Lake. A great amount of anti-malaria work was undertaken in this district by the very energetic officer in charge, Captain Cassidy, who has described some of his experiences and endeavours in the little book by Willoughby and himself, entitled "Anti-Malaria Work in Macedonia among British Troops." It is probable, though no figures are available, that there was a definite reduction in mosquitoes as a result of the measures adopted here. At any rate Janes, at the edge of the plain, had little malaria. Yet in 1918, when on a visit to this district, I found numerous *A. maculipennis* breeding in pools along the channel of the Gjol Ajak south of Kilindir and even in old oil cans belonging to the kite balloon section. And again in the small river west of Kilindir the same mosquitoes were breeding in a stretch of a mile or more which was inspected. A site on the banks of this river had been selected as a convalescent camp, and *A. maculipennis* was taken in buildings close by.

Another district in which anti-mosquito measures were strenuously undertaken was in the Dudula area on the Monastir road between Salonika

[1] A very good account of anti-malaria work in Macedonian hill streams is contained in a paper by J. A. Delmege, "Some Practical Notes on the Prevention of Mosquito Breeding" (*Journ. Trop. Med. and Hyg.*, October 1, 1919).

and the Galiko river and beyond it. Large numbers of troops occupied this area—the large ordnance depot, the railways, motor transport, concentration camps, hospitals and endless other units. The ground was cut up by numerous streams which formed marshy tracts in the low-lying ground. Canals were cut, the marshes drained, pools filled in and constant supervision rendered it so far mosquito-free that it could be taken that most of the mosquitoes which appeared had migrated from the districts beyond.[1]

The beautiful Hortiak plateau on account of its elevation was selected as a suitable site for summer hospitals. Here again the country was hilly and the plateau, itself high up, was surrounded by hills, with countless streams draining on to it. Very energetic measures were adopted here in 1918, as the year before there had been many cases of malaria amongst the hospital personnel. In spite of this mosquitoes remained very prevalent and could always be caught in numbers in the hospital marquees. As an illustration, the following catches were made by Captain Cummins at one of the general hospitals on the plateau:—

July 23 to 31	..	*A. superpictus*,	1,472	.. *A. maculipennis*,	10
August 1 to 31	..	,,	3,909	.. ,,	42
September 1 to 25	..	,,	3,910	.. ,,	59

These figures represent only a very small fraction of the total number of mosquitoes which had remained in the tents in the morning. In this district it was impossible to deal with all the streams in the surrounding country—this would have required thousands of labourers—and the mosquitoes constantly migrated to the treated area occupied by the hospitals.[2]

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[1] Beyond the Dudula area, where most of the energetic work was carried out, and about three miles farther along the Monastir road near the bridge over the Vardar was the 41st General Hospital used mainly for the treatment of Serbs. This hospital was surrounded by mosquito breeding ground and, as was to be expected, was highly malarious. For 1917 the hospital staff gave the following figures for malaria: officers 12, sisters 39, personnel 132. The incidence of malaria amongst the nursing sisters throughout the force was fairly high. Apart from those exposed to infection on night duty there was always a great chance of their being bitten by mosquitoes on the ankle and leg. Mosquito boots were rarely worn, and for some reason best known to themselves women have a great reluctance to overclothe this part of the body.

[2] The Hortiak pleateau, 2,000 feet above the sea, was a lovely spot which on account of its lower summer temperature was selected as a site for summer tented hospitals. The prevalent mosquito was *A. superpictus* and the figures of malaria incidence amongst the hospital staffs showed that even at this elevation there was no immunity against the disease. The 48th General Hospital was half way up the Eurenzik road leading to the plateau and thus at a lower level. In 1917 two officers and eight sisters suffered from malaria. Amongst the personnel there were 3 cases in July, 22 in August, 31 in September, 3 in October, 1 in November, and 1 in December. On the plateau itself the 61st General Hospital had 49 cases amongst its personnel and 5 amongst the sisters, while the 49th General Hospital lost from its personnel from June to December the following numbers: 1, 2, 26, 39, 13, 5 and 2. In August 14 sisters went down with malaria, in September 15.

Again at Guvezne, the busy railhead north-west of Lake Langaza, was a district at the foot of the hills rising along the Seres road. A large amount of anti-mosquito work was carried out, yet from the beginning of September to October, when the clearing measures were well advanced, thousands of *A. superpictus* were seen and collected from the hospital tents. It is probable that here also the mosquitoes were not locally bred but had descended from the hill streams which lay above Guvezne.

About September 24 a field ambulance moved on to the site just vacated by a casualty clearing station at Lahanah. The streams around the area had been constantly treated and watched and practically no mosquitoes were breeding within half a mile at least of the site, yet on the day following its arrival over 700 *A. superpictus* were collected from the seven marquees of the newly arrived field ambulance.

These illustrations serve to show that anti-mosquito work, even in districts which were not the most difficult to deal with, failed to eradicate the mosquitoes. So many were still present that it is a question to what extent, if any, infection was diminished by these measures. It can be well imagined that in the more difficult Struma valley and along the chain of lakes in the front line clearing work applied to the merest fraction of the whole can have only very slightly reduced the number of mosquitoes.

The spread of mosquitoes over long distances was not realized at the beginning of the campaign. They were believed to fly not more than a quarter of a mile and a clearance of half a mile around a camp was considered sufficient to protect it. The mosquitoes spread for at least a mile and it may be two or three miles when a gentle breeze carried them. Of this there is ample proof. For example on the hills west of Ardzan lake with a gentle south-easterly breeze large numbers of *A. maculipennis* and *A. sinensis* would travel up the hills and invade the tents of the units stationed there. These mosquitoes did not breed in the hill streams but only in the marshes along the shores of the lake. Similarly it was found near the mouth of the Struma that the galleries occupied by our troops on the low hills overlooking the swamp were infested with large numbers of *A. maculipennis* which could only have come from the marshy district at least a mile away.[3] A mile limit would in many cases prove sufficient but

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in October 6 and in December 2. At another hospital (37th General) stationed at the foot of the hills at Vertikop about eighty kilometres west of Salonika the malaria incidence was high. The mosquitoes here were *A. maculipennis* from the plain and *A. superpictus* from the hills. In 1917 amongst the personnel, non-commissioned officers and men, there were forty-five primary cases and sixty-nine secondary. In 1918 the figures were 55 and 94, giving totals of 114 and 149 for the two years. In 1918 there were twenty-three primary cases and six secondary amongst the sisters. In 1918 the establishment had been reduced to 210.

[3] The position at this point was in low hills on the left bank of the Struma near its opening into the sea but the galleries were constantly infested with mosquitoes which wandered up from the marshes below. The incidence of malaria was very high here, this

even that will not exclude the mosquitoes which wander farther afield. The half mile clearing limit round the units separated from one another by two or three miles left tracts of untouched breeding ground which rendered the measures almost valueless.[1]

It is probable that in certain areas some degree of infection was avoided as, for instance, along the Monastir road, round about Janes and in the Doiran-Vardar section, but even here this must have been very slight. In most places little or no good was done and it probably would have been better to concentrate energy on direct protective measures. The enforcement of the clearing work had, however, one advantage. It was an object lesson to everyone of the dangers of the mosquito, and taught each man

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being one of the worst spots in the line. In 1917 the 2nd King's Shropshire Light Infantry occupied this position for eighteen days and the resulting admissions to field ambulances were 190 during eighteen days. On July 14 the King's Shropshire Light Infantry were relieved by the 4th Rifle Brigade, who occupied the position for eighteen days. If two weeks is allowed for incubation then during the following eighteen days the admission to field ambulance was 146. That the admissions did not show the actual state of affairs as regards malaria the following figures will prove. On August 31, I visited the line here known as the Neohori position. There were two Battalions in the trenches and galleries along the low hills, the 4/60 King's Royal Rifles and the 4th Royal Brigade. Amongst the King's Royal Rifles there were 360 men known to have had malaria and who were taking quinine twenty grains a day, the idea being to give them this course for three months to prevent relapse. (All the other troops were on quinine ten grains a day.) On the day of my visit there had been a sick parade of about 100, practically all malaria. Only a very few of these were sent to field ambulance. The condition of the Royal Brigades was very similar. The Battalion of the Shropshires, who had just left the line for rest, was also visited. Here again the old malaria cases received quinine twenty grains a day and the other men ten grains a day. Of this Battalion 240 men sent away to field ambulance with malaria had not yet rejoined the unit, 174 men were old malaria cases and on quinine twenty grains a day. There were thus 414 malaria cases in the unit and only 485 who were not known to have had the disease. The King's Shropshire Light Infantry who were also on reserve had sent 390 cases to hospital between June 16 and December 12. In these units in the Neohori position an attempt was made to keep the men in the Battalion and many were retained who otherwise would have been sent to the field ambulance. A man was kept unless his temperature persisted for over forty-eight hours. This procedure was abandoned owing to the fact that some of the cases which were retained developed serious cerebral symptoms which could not be properly treated out of hospital.

[1] It is now generally recognized that mosquitoes will travel very long distances, even up to ten miles. In this connexion it is worth while to record an experience in Mesopotamia. The deep draught steamers running up the Persian Gulf, on account of a bar across the mouth of the Shat-el-Arab, had to anchor ten miles from shore and transfer their freight to shallow draught steamers. On one occasion when anchored here a breeze from the shore brought on to the ship thousands of house-flies, dragon-flies and other insects which must have travelled ten miles over the sea. I was informed by a medical officer on a hospital ship running on this route that on one occasion a similar invasion by mosquitoes —*A. pulcherrimus*—had taken place. I believe this observation has been recorded by Major R. E. Wright in a paper entitled "The Distance Mosquitoes can Fly" (*Bombay Natural History Society*, xxv, No. 3, 1918).

that the bite of a mosquito, which could necessitate such an expenditure of labour, was something to be avoided.

Quinine Prophylaxis.

The question of quinine prophylaxis is a very controversial one. Undoubtedly the concensus of opinion in Macedonia was that it was worthless. Still there were some medical officers who believed that the men under their care were protected from infection by the regular daily dose of the drug. Exact figures are very difficult to obtain and a great deal of confusion has arisen as to the precise meaning of the term quinine prophylaxis. It certainly covers two distinct conditions—the dose of quinine which may be expected to prevent infection and the dose which will prevent an attack in an already infected man. The former is the true meaning of prophylaxis and it is generally assumed, though there is no evidence to support it, that the quinine circulating in the blood destroys the sporozoites directly they are introduced by the mosquito. If quinine actually prevents evident infection it may very well be that the sporozoites are untouched, that they invade the red blood corpuscles in spite of the quinine and that cure is effected by the quinine before the parasites have reproduced sufficiently to bring about an attack. If this latter view is correct then prophylactic quinine acts by treating the very earliest phases of infection and this is in line with the generally accepted view that the earlier a case of malaria comes under treatment the better is the chance of success.

Another question which requires an answer is the relation of the dose of infective material to the amount of quinine required to prevent infection. It is quite possible that prophylactic quinine will prevent infection in a man who is only occasionally bitten by infected mosquitoes, and that it will not do so in cases where daily injection of large numbers of sporozoites occurs. This may be merely due to the fact that, even when very great care is exercised in giving quinine daily, intervals of a few days are bound to occur when quinine is not taken. Though this must have happened very frequently in Macedonia, yet there were thousands of instances where infection first showed itself in men taking quinine quite regularly. There are no figures to show what percentage of these were certainly not infected before commencing the prophylactic quinine. That a daily dose of quinine will kill sporozoites injected, say, once a fortnight, and not kill the larger number injected every night, or even ten times nightly, is difficult to understand. However large may be the number of sporozoites injected in a single night, compared with the whole volume of blood plasma containing quinine they are but the smallest particle. If the Atlantic Ocean were poisoned so that a hundred fish were killed when introduced, it is inconceivable that the result would be any different if, instead of a hundred, a thousand times this number, or more, had been exposed to the poison, unless we assume that there is an occasional fish here and there with an idiosyncrasy against the drug. We do know that quinine will cause the

disappearance of the vast majority of the malarial parasites in the human body, but that some survive is proved by the frequency of relapses. This may be due to the relative insusceptibility of a small number of the organisms. This is certainly the case in trypanosomiasis, in which, if the forms susceptible to a drug are constantly killed off, more and more insusceptible forms appear, till finally, by a process of natural selection (the survivors always producing larger numbers of resistant forms), a whole race of insusceptible trypanosomes results.

If we assume that the malarial sporozoites are similar to the blood forms, as regards the action of quinine, then there will be a small number of sporozoites which will not be killed by the drug taken prophylactically. There may be only a few resistant forms amongst the total sporozoites injected by a number of mosquitoes. If an individual is being bitten by only a small number of infected mosquitoes, the chances of his acquiring malaria will be very much diminished if he has quinine in his system, for it will be only very rarely that resistant sporozoites will be injected. If, however, he is being constantly bitten, he will almost certainly be infected, for amongst the large number of sporozoites injected there will be some resistant forms. This, of course, is highly speculative, but it will explain the view that infection is more likely to occur when the sporozoite dosage is high, and also that quinine will act better as a prophylactic when the exposure to infection is not of long duration, as, for instance, during a short stay in an infected area. When exposure extends over long periods, and the incidence of infected mosquitoes is high, then prophylactic quinine will have little value.

It is, nevertheless, true that in Macedonia quinine failed to prevent infection in thousands of cases, but there is again nothing to show that the quinine did not protect at least a small number of individuals. The incidence of malaria was so enormous that it is supposed that without quinine it could hardly have been any higher. Was the expenditure of money and labour involved in this administration of prophylactic quinine on so vast a scale worth while? The concensus of opinion was that it was not, and my personal opinion agrees with this.

On the other hand, the regular administration of daily quinine undoubtedly acted by preventing relapses in already infected men. To what extent this occurred is difficult to judge. Furthermore, this suppression of a manifest infection had the immediate advantage of keeping men in the line, but sooner or later attacks of malaria occurred, the treatment of which was more difficult on account of the previous continual dosing with quinine. Here again the question arises whether this was due to an acquired resistance by the parasite, the isolating by selection of a particular quinine-resistant strain, or whether it was due to some acquired capacity of the body to get rid of or store up quinine so that it failed to reach the parasites at all. The bad effect on the general health of an individual of continuous dosage with quinine over long periods has to be considered.

In the winter of 1916-17, when the investigation into the number of carriers was being made, the regular administration of quinine was temporarily stopped to render the finding of malaria parasites more easy. The result of this was an immediate increase in the number of malaria cases. The quinine was accordingly recommenced and the malaria rate fell.

The amount of prophylactic quinine given varied at different times and places. Five to six grains a day was the dose chiefly given in 1916—10 grains a day, 10 grains on two days of each week, 20 grains, and even in some units 30 grains a day was tried. Sometimes a double dose was given on one day a week. None of these methods of preventing malaria gave any encouragement. In one case a very large body of men in the winter of 1917-18 were placed on a twenty-four days' course of quinine—thirty grains a day, combined with iron and arsenic tonic. It was hoped that a large proportion would be cured of their infection. The immediate effect was a fall in the malaria admission rate, but this gradually mounted again till before the course was completed it was as high as it had been originally. After the course the figures were higher still. This experiment was an attempt at cure, and was not truly a prophylactic measure.

It may be argued that no really scientific test was made with groups of men under identical conditions with and without quinine, though this was attempted. Men previously infected and men with no infection received quinine alike but there was such a mass of material to observe that the general view that quinine did very little, if anything at all, to lessen malaria amongst the troops in the line must have been correct. It is highly improbable that any variation in the time of administration of the prophylactic dose would have affected the result. As it happened, the quinine was most usually taken, at any rate in 1917 and 1918, as an orange-flavoured solution of quinine sulphate in the evening, before the evening meal, and this is the time which is considered to be the most favourable, so that the greatest concentration of quinine in the blood may be at night.

We have considered above the anti-larval measures adopted and quinine prophylaxis and both of these were disappointing in their results. The third and last method of avoiding malaria is that of the protection of men from the bites of mosquitoes. In 1916 very little was done in this direction; in 1917 there were great advances, while in 1918 the state of affairs was still further improved.

Mosquito nets, of all methods of malaria prevention in case of emergency, such as occurs when troops are moving about, are of the greatest importance. By the proper use of a net, provided the individual can remain within it all night—not always possible of course in time of war—the number of mosquito bites can be reduced to nil. It is my opinion that the mosquito net did more to prevent infection than all the other methods of malaria prevention together. Head nets and gloves were of limited use though they were definitely advantageous in certain places. In the front

line for men on guard duty or patrol they obscured vision too much, though they were used and must have prevented many mosquitoes from injecting sporozoites. In the hospital tents, however, they were worn with benefit by the orderlies who were on duty all night. For instance, at a casualty clearing station at Karasuli the tents were full of mosquitoes at night, especially when the wind blew from the direction of Ardzan or Amatova lakes. Here to remain in a tent long without protection meant certain infection. Mosquitoes had to be constantly brushed off the neck and hands. The head net and gloves could be, and were, used with advantage here. It was at Karasuli that the importance of the proper use of the mosquito net was brought home to me. I had examined a man during the day and found his blood harbouring numerous crescents. On visiting him at night he was asleep and a patch of bare skin lying against the net was simply covered with *A. maculipennis* and *A. sinensis.*

FIG. 7.—Bivouac mosquito-net as supplied to the troops in 1918. The net is intended for two men and is covered by the two bivouac sheets. The front slides up and down the guy-rope.

Large numbers of mosquitoes were collected from the tents all over Macedonia and it was always a most disconcerting feature of such collections that a very high percentage—sometimes well over seventy—of the mosquitoes had evidently fed during the night. This can only have occurred because the nets were not properly used or were themselves unsatisfactory. Of mosquito nets there were mainly two types. The bivouac net was modified and altered till in 1918 a very serviceable pattern was evolved. Each net was devised to take two men who entered at one end, which was weighted so that it automatically fell to the ground as it ran on rings along the front guy-rope. From the point of view of mosquito protection a single smaller net of the same pattern for each man would be a distinct advantage. The other type of mosquito net was that used in buildings, tents and hospitals. This was the bell net pattern fitted to a wooden hoop hanging over the head. These nets are difficult to

keep adjusted and it appears to me that the rectangular net with the calico strip around the base would not only afford a greater protection but would be more economical of material. The fixing of these nets in parallel rows on cords would not be a difficult matter. A further point is that the supply of nets in each unit should exceed that of the number of men, so that a damaged net could be at once handed in for repair.

Fig. 8.—Mosquito-proof shelter near the Doiran front.

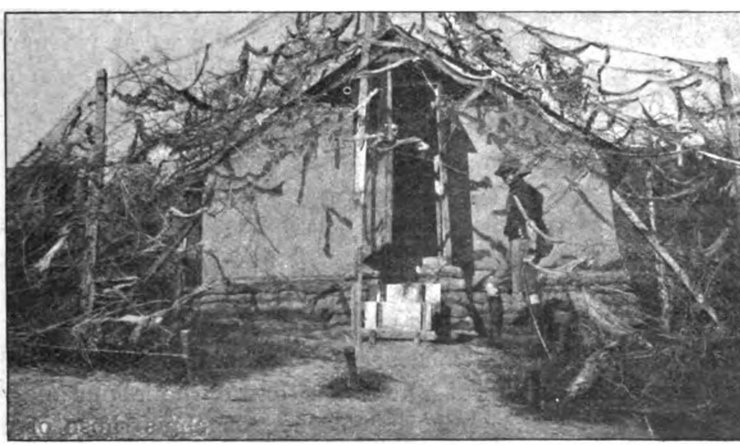

Fig. 9.—Camouflaged mosquito-proof shelter near the Doiran front.

Repellents of various kinds were generally employed. It was recognized that for men at work or on patrol in the Struma valley or other parts these had very little effect in preventing mosquito bites for longer than half an hour after application. Apparently it was the grease, rather than any ingredient, that the mosquitoes did not like and when this had been washed away by the perspiration which streamed from the men in the hot summer

nights the mosquitoes renewed their attacks. The men accordingly took tins with them and the application was renewed at intervals during the hours of watch or patrol and probably did something to diminish the number of bites.

During 1917, and especially 1918, a great deal of screening of buildings, galleries and dug-outs was carried out, and in 1918 a large number of light, mosquito-proof huts made of canvas, wood and gauze were put up so as to enable the men to enjoy recreations without the danger of being constantly bitten. These were most useful, as the men could sit in them before retiring for the night. The benefit derived from these huts must have been very great and one or more of them, according to the size of the unit, might be considered as part of the necessary equipment wherever it would be possible to use them. In some places mosquito-proof wards were constructed in the hospitals as, for instance, at a casualty clearing station at Karasuli, and in these malaria cases were not only protected from the mosquitoes more effectively than by the use of mosquito nets alone in tents, but were treated more efficiently and with less danger to the attendants at night. Such wards could have been adopted with advantage on a much larger scale, for it is just as important to protect the infected individual from uninfected mosquitoes as the uninfected individual from infected mosquitoes.

There were many minor devices, such as the turn-down shorts to protect the knees at night, mosquito swats to kill the mosquitoes in the tents in the morning—a very important point, for many of them were full of blood —sprayers for various fluids to kill mosquitoes, fumigation, mosquito traps, the formation of trap pools and numerous others. It is impossible to consider all these and indeed they do not differ from the devices used in anti-malaria work elsewhere.

Looking at the whole campaign against the mosquito one cannot help feeling that under the conditions which actually prevailed in Macedonia—and this is probably true of any campaign in a highly malarious country—if all the energy and money expended had been directed entirely to the protection of men against mosquito bites the malaria would not have been more, but probably less, than it was. In the case of a campaign in a mosquito-infested land these measures alone, if thoroughly carried out, would give a greater degree of protection than if the labour and expense were divided amongst this and other methods, such as those of clearing, or quinine prophylaxis, which may be very useful in times of peace or permanent occupation but have proved disappointing in Macedonia in time of war.[1] The money expended on prophylactic quinine alone would have

[1] In this connexion a paragraph from James's most interesting and opportune book on malaria ("Malaria at Home and Abroad," p. 199) is of interest:—

"Perhaps we must make allowances for the frailty of man as well as for the wiliness of the mosquito, but whether this is true or not, practical experience is to the effect that it is usually best to concentrate all available effort on one carefully selected method which can then be brought to a high degree of perfection."

supplied thousands of mosquito-proof huts and hospital wards and almost unlimited quantities of necessary gauze. It would have given each man a separate mosquito net instead of compelling him to share with another, and would have enabled these to be replaced at once when damaged—always supposing of course that the necessary materials could have been obtained, and they probably could have been if their necessity had been insisted upon. A sufficient supply of all these things should be regarded as part of the necessary equipment of an army entering a malarious country. A mosquito net to protect against the mosquito is as much a necessity as the sun helmet to protect against the sun.

I have noted a danger which has frequently arisen. In rest camps both for officers and men even when a sufficiency of nets was available there has been delay in their issue. Troops arrive late at night either by ship, train, or on the march, and it is often impossible to supply nets to the men till next day or even later. There seems to be no reason why the tents or buildings for their reception should not be arranged with permanently fixed nets, as in a hospital ward, so that protection can be afforded at once. The argument has been that the men bring the nets with them, but in practice this arrangement more frequently leads to exposure than otherwise. I quite realize that arrangements of this kind would require special organization and extra staff.

It has been mentioned above that the line west of our extreme left on the Vardar river was occupied by the French. They shared Salonika with us as a base and were exposed to malaria much as we were though, with the exception of part of the Vardar delta, they occupied more hilly country and escaped the terrible exposure of the Struma valley. Like us, the French suffered badly from malaria and they carried out anti-malaria measures, including the administration of prophylactic quinine much on our lines. I think it is true that they reduced their malaria admissions by sending away infected men, even during the period of unrestricted submarine warfare, more freely than we did. In a paper on malaria in Macedonia an account is given of the anti-malaria work carried out in the French Army in 1915, 1916 and 1917.[1] Prophylactic quinine was given generally and its administration was controlled by a system of inspection of the urine. At any time a sample of urine could be demanded of a man, and this was tested with the Tanret reagent for the presence of quinine, which should be invariably present if the daily dose of prophylactic quinine was being properly administered. Mosquito nets were provided for the troops, but never to the extent seen in our Army. Mosquito-proofing of buildings, dug-outs and other dwellings was undertaken, but here again this protective measure was developed on a much larger scale in our own area. Similarly, anti-larval measures were carried out energetically in

[1] "Travaux et résultats de la Mission Antipaludique à l'Armée d'Orient." *Bull. Soc. Path. Exot.*, tome xi, No. 6, 1918.

certain localities, but the British Army did this work on a much larger scale and more universally than did the French. We have seen how difficult it is to judge of the effect of these measures, for we had a greater amount of malaria in 1917 than in 1916, yet the paper now under consideration states on p. 469, that it is easy to realize the enormity of the task which was imposed on each sectional medical officer, a task, however, which was perfectly carried out on their part, as the following results show:—

For every sixty cases of primary malaria occurring in 1916, there were only seven in 1917. That is to say that all the improved measures adopted by the French in 1917 had reduced their malarial infections by about 90 per cent. We are not told how this figure was arrived at, but after our own experience one cannot help feeling that it is a very liberal and optimistic estimate of the effect of their anti-malarial measures.

In a paper written by four medical officers of the Macedonian army,[1] some figures of the incidence of malaria are given. For the period, December, 1915—December, 1916, inclusive, there were 31,727 admissions to hospital for malaria. Of these 17,614 were evacuated to France, and 667 died, leaving 13,446 in the country. Of the total admissions about 19,000 were regarded as primary attacks, and 12,000 as secondary or relapse cases. In the period January to November, 1917, the number of admissions was 34,544, of which about 3,000 were considered primary cases, and 31,000 secondary. It is difficult to reconcile these figures, for if at the end of 1916 there were only 13,446 known cases of malaria in the army, how is it that in 1917 there were 31,000 cases of secondary malaria? The only way the figures can have been arrived at would be from the statement of individual soldiers, who would maintain that they had had previous malaria but had not been admitted to hospital. An attempt was made in our own army to obtain such information, but it was found so unreliable that it had to be abandoned. No account is taken of possible recovery and reinfection, so that it appears to me that the estimate of the number of primary cases for 1917 must be far too low. The general figures, however, agree with those of the British Army, for there were more admissions for malaria during 1917 than in 1916.

The Serbians suffered badly from malaria, though probably not so badly as we did, as they occupied a more hilly country. Our enemy, also, was not immune, for I was informed by a Bulgarian doctor after the armistice that at one time there were 23,000 in hospital with malaria.

(*To be continued.*)

[1] "Le Paludisme," par MM. Niclot, J. Baur, Monier-Vinard, et M. Beguet, Macedoine, 1915-1916-1917.

THE VISION OF THE SOLDIER, WITH SPECIAL REFERENCE TO MALINGERING.

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(*Concluded from p.* 49.)

V.—Blindness in One Eye, Actual or Alleged.

We now come to consider that group of cases in which it is alleged that there is blindness in one eye.

It will not have escaped notice that in the preceding chapter the only piece of apparatus referred to, apart from the ophthalmoscope and trial-lenses, was Bishop Harman's diaphragm test. The reason for this is that the less the medical officer depends upon mechanical appliances in the detection of alleged visual defects the better. In the civil courts, of course, he must be prepared with an answer when counsel, to make the most oı himself, springs on him the question, "Did you try the plaintiff with so-and-so's apparatus?" mentioning a complicated affair, invented, tried, found wanting, and scrapped. But in military ophthalmology, the three instruments, combined with the medical officer's own sleight of mind, are all that are necessary.

Here again, the ophthalmoscopic examination may immediately solve the problem. There may be present that definite, visible lesion to which reference has been made, which will confirm the patient's statement. But if the fundus shows no gross changes, no mydriatic being used in the first instance, the history, especially if there has been an injury, must be scrutinized in the smallest detail. The eye may be amblyopic from want of use; there may be evidence of an old operation for strabismus or for advancement; the pupillary reaction should be noted.

War injuries in particular demand the closest investigation as to their nature and extent: there may be commotio retinæ without external wound; concussion cataract may appear some months after the date of injury. These points, of the highest clinical interest, may severally support a soldier's statement that he has completely lost the sight of one eye, and the medical officer's duty is to verify that statement apart from the alleged cause.

The tests should be of the simplest. Bishop Harman's may prove binocular vision at the outset and so save much trouble. But if one eye appears with it to be amblyopic, which eye it is should be noted. In a case of malingering, the test may be used again in course of the examination, and perhaps show this time that it is the "good" eye that is now amblyopic.

Hazelberg's test-types, in which the letters are partly red, partly black,

are based on the fact that a red object by reflected light is invisible to the eye looking through a red glass of the same value. The difficulty is to obtain a correct balance between the tones of the printing ink and the glass, for if the ink is not exactly neutralized it may appear grey, and so defeat the test. The black letter C is converted into an O by the addition of a red segment. The black P has a red tail added to it to make it an R. Similarly, F becomes E, V becomes W, and so on. The soldier stands at the proper distance with the red glass before the "good" eye and some lens of very low power before the "blind" eye. When this is done, and not until it is done, the test-types are exposed. Here a careful watch must be kept on the man's eyes, and on the slightest attempt to close one, the medical officer's hand should quickly cover the trial-frame. The soldier, ignorant of the physics of the test, imagines that he sees the red part of the letters with the red glass before the "good" eye. If the "blind" eye is really blind, the red glass before the "good" eye will obliterate the red segments so that he will read C P F V. But if there is sufficient sight in the "blind" eye, he will see the red segments and the letters will be read as O R E W. When a result has been obtained, the red glass and test-types should be removed out of sight to prevent curiosity.

For the test with transparent red and green letters the trial-frame is fitted with disks of the same colours, the physical fact being that a transparent green object becomes invisible when looked at through a red glass. Thus if all the letters are identified, irrespective of their colour, there must be sight in the "blind" eye. Care should be taken beforehand to see that the reds and greens *do* negative one another. For the too familiar FRIEND, which reads either FIN or RED, may be substituted the somewhat cynical and suggestive TRUANT CHAINS.

Tests with prisms are used in the following ways:—

(*a*) A prism base downwards is placed before the "good" eye while the man looks at a distant candle-light. If he sees two lights, binocular vision is present.

(*b*) A prism of ten degrees with the base outwards is placed before the "blind" eye. If there is any sight in this eye diplopia will be produced, and the eye will move inwards to correct it and fuse the two images.

(*c*) The "blind" eye is covered. A prism of ten degrees is placed before the "good" eye in such a position that its apical edge lies horizontally across the centre of the pupil. This produces monocular diplopia. The prism is then moved up so as to be completely in front of the "good" eye, and the "blind" eye is uncovered. If diplopia is produced or admitted, there is sight in the "blind" eye.

This test may fail owing to the difficulty of placing the edge of the prism in position without some preliminary adjustment, during which the man may be on his guard from having seen single at one moment and then double through the same glass. It demands intelligence on the part of the patient, and may defeat its object.

(*d*) Maddox's double prism consists of two prisms of twelve degrees ground out of a single piece of glass with base to base. When it is held in front of a seeing eye so that the sharp line formed by the junction of the two bases is exactly horizontal in front of the pupil, monocular diplopia is produced, the other eye being covered. Thus, when a candle-flame is the object, the eye sees two, one above the other. If there is sight in the other eye, now uncovered, a third object is seen somewhere between the two. In making the test, both eyes are kept open, and the double prism is adjusted before the "good" eye, the medical officer's hand somehow getting in front of the "blind" eye. When all is ready, the man is told to look at the candle. If he sees three, there must be sight in the "blind" eye.

A neutralizing test may be employed, but in this the medical officer's eyes should never be taken off the examinee's for an instant. The trial-frame is fitted with a + 10 D. in each, both eyes are to be kept open, and the trial-case so placed that the medical officer can find the required lenses by touch without having to look for them. The man is told to ignore the "blind" eye and say what he sees with the other. Neutralizing then begins, and the scale is gone up with minus lenses moving at the rate of quarter dioptres for the "good" eye, and whole dioptres for the "blind" eye. If the scheme succeeds, there should be about − 2 in front of the "good" eye, and − 8 D. in front of the "blind" eye. Thus he will still have a high plus glass for the "good" eye, and a low plus glass for the "blind" eye. If he reads anything it must be with the "blind" eye. The success of the test lies in the man never being allowed to close one eye. If, however, he attempts to close the "good" eye, the evidence against him is all the stronger.

The trial-frame is adjusted with + 10 for the good eye and a low + glass for the "blind" eye, it having been ascertained beforehand that the man is emmetropic or slightly hypermetropic. He is then told to read ordinary print. If he does so at twelve or fifteen inches, he must be seeing with the "blind" eye, for if it were really blind he would bring the type to about four inches from his face so as to read with the "good" eye. If he saw with the "blind" eye, he naturally would prefer the more comfortable reading distance to which he was accustomed.

In the case of a myope, these tests would be applied after he had been made approximately emmetropic with his correction.

Another neutralizing test can be carried out with cylinders. The trial frame is fitted with a plane lens for the "blind" eye, and a plus and minus cylinder of about 2 D. for the good eye, their axes being parallel. The man is told to read. While he is doing this, the medical officer, by way of adjusting the trial frame, "accidentally" jerks one of the cylinders so that its axis is at right angles to the other. If the reading is continued, the man must be using his "blind" eye.

Advantage may be taken of the limitation of the visual field to the nasal side. The man is examined in the dark room. Both eyes are kept

open. A candle-light is held directly before him, and then moved slowly towards the side of the "blind" eye. If the light is still seen when it is in such a position that it is cut off from the "good" eye by the interference of the nose, there must be sight in the "blind" eye.

Similarly the consensual pupillary reaction affords evidence of vision. When one eye is absolutely blind, there is no pupillary reaction, but in doubt the "blind" eye should first be covered, and then light admitted to it. If the pupil of the "good" eye reacts, there must be sight in the "blind" eye.

In all these tests the difficulty of proof is the greater if the man has already been examined by means of them. In any case it is advisable for the medical officer to begin by saying, "We won't bother about the 'blind' eye just now. I want to see what you make of this with your 'good' eye."

Functional or "psychic" blindness associated with war-neuroses has claimed the attention of clinicians. In this there is not as a rule an ascertainable organic lesion, but there will most frequently be a history of some form of functional paralysis. This should be investigated. The patient may be so convinced that he does not see with one eye that the possibility of being suspected of malingering does not occur to him.

With shell-shock cases, in which there have been concomitant disturbances, it is well to begin by assuming, or pretending to assume, that the defect is genuine but curable, and to proceed on the lines laid down by Yealland in his "Hysterical Disorders of Warfare," p. 52.

Although treatment does not concern us here, it cannot be dispensed with in eliciting the facts. Here, once more, it is the medical officer who first sees the case, who has the best chance of obtaining a good result, but as it is unlikely that a man suffering from "psychic" blindness has not already been examined by others who have lacked the patience or the insight to study and appreciate the mind of the patient, the tests may fail, and a further examination may be necessary, the patient meanwhile being if possible kept under observation.

It sometimes happens that monocular blindness is ascribed to a hysterical disorder which has passed off, if indeed it ever existed. The diagnosis will then rest upon the exclusion of fundus changes, and the application of the tests mentioned. The case then passes into the category of assumed blindness of one eye and the "psychical" element may be discounted, except in so far as most if not nearly all malingering has a psychic foundation.

So much has been published on the neurological results of war that a malingerer with some intelligence will not have difficulty in informing himself of a plausible chain of symptoms and the tests to which he will be subjected. Acute observation of the man himself, with the alleged blindness ignored for the moment, and a close interrogation upon apparently trivial side-issues, may lead to a clue.

When malingering is suspected, the motive must be sought for. Since

this class of case usually is found among soldiers who have undergone the severest strain to which the mind and body of mortal man can be subjected, the benefit of the doubt, when doubt is present, must be in favour of the patient.

But the idea of monocular blindness may be due to suggestion, to unconscious study of others, to the comparing of notes, to inquiries as to personal experiences of warfare, to the open examination of others by expert medical officers.

The motive may be pecuniary, grasped at in one case, scorned in another. It may be born of misdirected sympathy, of undue familiarity and association springing from joy-rides and tea-parties with much be-photographed "war-workers." The future too has to be taken into account, the prospect of honest labour, the willingness to shed the stars and crowns of temporary service for the well-earned stripes of toil.

The examination of the mind, with the vision as an after-thought, will serve to distract the attention of the patient sufficiently for the sudden surprise-test. In all examinations the medical officer has to drag the patient out of himself, he has almost to change places with him so that the patient begins to study his examiner curiously and be interested in him. But in the midst of apparent irrelevancies the main issue must never be lost sight of, and though the patient may at times feel anxious about the medical officer and think him in worse mental plight than himself, he must not be allowed to see that there is "much method in his madness." What the surprise-question may be will depend on the examiner's mother-wit and his assumed air of thoughtlessness, leading the patient to imagine that the medical officer is off his guard. Thus the medical officer's carefully calculated indiscretion may entice the patient into a damning admission.

Diplopia.

It is unlikely that a man will have sufficient knowledge to assume with success the symptoms of diplopia, but he should be examined as if his complaint were genuine, every precaution being taken to guard against a serious nervous lesion being overlooked. The medical officer must be prepared to exercise the utmost patience, for even when a paralysis does exist, the man may not always be ready with his answers.

It is a good plan, and sometimes saves time, to make diagrams of the various positions of an object, such as a candle or electric bulb, representing each form of diplopia, and to ask the patient to indicate which corresponds with his double vision. If he is malingering he will be puzzled to find that there are several ways of seeing double, and whichever diagram he chooses can be verified by the tests given below. After the routine examination of muscular balance, media and fundi, a close study of the clinical history should follow. A red glass is placed before one eye with the electric bulb in the dark room as the object, or, better, a strip of white

8

paper, twelve inches by two, pinned vertically on the black wall. The man is then told to describe what he sees. If he is malingering, he may say that the false image is vertical and the true image oblique. The red glass is changed over to the other eye and again he is told to describe the position of the white and red objects—if he is seeing double. The malingerer always sees double, no matter what the direction of the eyes may be. The typical turning aside of the head and some lame explanation may be expected at this stage. With the "good" eye covered he is told to touch the examiner's hand or to pick up a piece of paper from the table. In a genuine case he fails, and false projection is proved. Told to go up or down stairs with his good eye covered, if diplopia is present he will say he dare not. When there are grounds for doubting the man's statement, and when the existence of graver lesions has been excluded, the safest treatment is none. If he is malingering, he will not undergo any inconvenience; if he makes his complaint in good faith, the consistency of his answers and his anxiety to see well will decide the question.

Monocular diplopia is rarely met with, but it exists and can be particularly disconcerting. In one case, that of a medical officer who had over-long hours with the ophthalmoscope, it usually made its unwelcome appearance in the right eye towards the end of the day's work. It took the form of a faint grey duplication of every object. With Snellen's types each letter was seen twice, the pale second image being about halfway up the black. There were no lenticular opacities, and it vanished on the addition of a + 0·75 cylinder to his hypermetric correction. In the soldier it may be disregarded, unless he has been occupied daily and hourly with microscopic or ophthalmoscopic work—a state of affairs scarcely likely to occur. In any event, however, the routine examination should not be neglected.

A soldier who complains of hemianopia must, if malingering, be possessed of all the knowledge of his examiner and possibly a good deal more. His statement, when investigated, will without much difficulty, decide its credibility. The perimeter and stereoscopic X-ray plate will clear up doubts, along with the evidence of occipital injury. The calcarine fissure is a region not in the day's journey of everyone.

A case may be mentioned here in which hemianopia, though genuine was used for fraudulent purposes. It was in the days when some looked upon the perimeter as a toy, and Wassermann was an unknown name. A man presented himself at an eye clinic for a certificate of vision, in order to claim a pension from the Admiralty. He had been a draughtsman in the Navy. What was singular about him was his gaze. In conversation his eyes were fixed on a point over the observer's head. It was evident that he had lost the lower sector of his visual field, and the perimeter confirmed this. Lesions existed in both fundi, and the case was dealt with as specific. The certificate was refused. About eighteen months later, he came to the

same clinic for the same purpose. He was recognized by the fixation of the gaze, but denied that he had ever attended the clinic. He further maintained that the name that he had given on the former occasion was not his. The medical officer being sure of his ground, did not make an ophthalmoscopic examination, but asked a colleague to do so, and then proceeded to describe to him what he was likely to see. The fundi had not altered and the colleague verified their condition. The man admitted —he could not do anything else—that he was the old patient.

Hemeralopia—to give the adopted but confusing name to night-blindness—has been identified with active service since the days of the Crusades. In plain language it means that a man cannot see in the dark. He is blind at night.

It may be accepted as true that everyone coming from a brightly illuminated room into sudden darkness sustains a retinal shock. This shock equally disables when the conditions are reversed, such as sudden emergence from shadow into full blaze of sunlight in one's eyes. There is a solar scotoma which may persist for twenty-four hours or may be permanent. Night-blindness may be induced suddenly by a physical cause, a slip of the foot in the dark, a want of solid resistance on a step into vacancy, an unexpected buffet in the dark from a comrade, binding the limbs and blotting out all vision. When complained of, it should not be regarded lightly, and the medical officer who would dismiss a case as imaginary, would take upon himself a serious responsibility in sending a man back to the line without a searching examination. There may be no abnormality discovered in the fundi, but the perimeter may afford a clue. It may be due to conditions of the media, to refractive errors uncorrected, especially in myopia. In some instances it may have been latent since birth, to be lit up by the exactions of service. A chain of heredity may be discovered stretching back for more than a generation. But its existence, even among men wishing to do their best, cannot be ignored. In warfare of position as opposed to movement, with so much depending upon operations to be carried out in darkness, a man with night-blindness is a danger to his comrades, with the added anxiety to himself that his vision may fail him at the critical moment. It will be generally agreed that a man affected with "retinitis pigmentosa" is unfit for the Army, but there may be greatly contracted fields without any pigmentary disturbance. Cases of malingering were rare at home stations. They were dealt with on whatever front they occurred, and were not given the chance of a transfer to home service. The only way by which malingering can be detected is to have the man watched at nights, or to question his comrades as to his behaviour in the dark. Dissimulation under these conditions cannot be kept up for long; the instinct for self-preservation is bound to assert itself and betray the deceit.

VI.—Examples.

Malingering is a disease due to a microbe of a particularly virulent type. Young and healthy men at clerical work in comfortable offices resist it for a time, but when there is a "comb-out" or a medical examination for a draft, it develops with alarming rapidity. The eye specialist should be in close touch with the regimental medical officer so that suspected cases or contacts may be isolated and dealt with at once.

The manner of the specialist will do much to stop the rot. It is in his own interests as well as those of the authorities that he should do so, for there is nothing more irritating than to find men pitting their brains against his. Personal acquaintance with the class of a man's work, as clerk or draughtsman, the lighting of the building in which he is employed, the conditions of service maintaining at the time, may put him on the track.

This chapter will deal with a few examples out of many.

Sometimes interference may come from an unusual quarter outside. A man had been conscripted, and within forty-eight hours his wife wrote to the local M.P. (Labour) protesting indignantly that it was disgraceful that a half-blind man should have been enlisted. The Member believed the story and gave notice that he would put a question in the House to the Under-Secretary of State for War. A type-written copy of the question was sent to an eye specialist, and the man followed. He turned out to be a flagrant malingerer and a report to that effect was passed to the War Office. The M.P. had been "had." The question was not put, and it is to be hoped that he "got something back" on the wife.

At one time numbers of young men were being sent from a certain office with extremely low refractive errors. It was a not uncommon device for men who had been clerks in civil life to get their sight tested by an optician who did not let them off till they had bought a pair of glasses, generally little more than half a dioptre with perhaps a cylinder of 0·25 D. These they produced as if to impose upon the medical officer and impress him with the presence of a visual defect. They were examined and dismissed with advice. But as they still kept arriving in parties of a dozen at a time, a "comb-out" was suspected. The latest comers were paraded and told that if any more like them appeared, the medical officer would have the entire staff medically examined at once. No more came.

In a large district the specialist had at one time to examine the eyesight of some hundreds of men belonging to what were then called provisional battalions. These were units made up of men who had been sent home from over-seas with various temporary disabilities. It struck the President of the Travelling Medical Board as somewhat odd that men sent up for a report on their visual defects no longer complained of their eyes at their next Board, but had something else the matter, and he

sought out the medical officer for an explanation. It was simple. The medical officer had invented an idiotic sort of jargon which he called his Riot Act, and this he solemnly read out to hardened sinners. Highly irregular, perhaps, but it worked.

A man had been sent up for examination with a view to his discharge. The President of the Board, rather doubtful of the good faith of the man, sent him to the specialist. A civilian practitioner's report was produced which referred to posterior synechiæ in both, but to nothing else. The pupils were small and partially blocked, but the man denied all recollection of having had acute pain, and this raised suspicion. He stated that he had been in the Army while between the ages of 19 and 23, and had rejoined when over 40. As his second term of service had been only ten months in duration, it was extremely unlikely that the iritis was of so recent date, and homatropin and cocaine were instilled. The pupils yielded irregularly, but sufficiently so as to allow the fundi to be seen, and extensive choroiditis, probably specific in origin, was found in both. The condition was of old standing, and not "in or by."

The importance of an immediate ophthalmoscopic examination in every case of injury, not only of the affected eye but also of the good eye, cannot be pressed too urgently. A man was brought up for consultation with the history that on the previous evening he had been struck on the left eye with a rope. He was in the Flying Corps, and the medical officer in attendance had put in a drop of atropin. There was no evidence of serious injury, but the man said he had never seen with the eye. This alarmed his medical officer who wisely sent him at once to a specialist. The media were clear, but the fundus was covered with masses of choroidal pigment which could not have developed in a night. Further, there was a patch of choroiditis in the macula of the good eye, which confirmed the opinion that the condition was of old standing and not due to the injury. But suppose the man had made light of the injury at the time, and a year later had exaggerated its effect in order to base a claim upon the blindness, he might have fallen into the hands of an examiner who possibly might have given him the benefit of the doubt and, without examining the other eye, decided that the choroiditis was traumatic. This case exemplifies the wisdom of the rule: *Examine both eyes.* The condition of the "good" eye may throw a light upon the alleged defects of the other eye.

The risk of accepting a man's or any statement without a full examination is shown in the following case.

Towards the end of August, 1914, Private H. had some sand and earth thrown up in his face from a bullet striking the parapet of his trench. As the debris had gone into his eyes he was hurriedly bandaged and removed to a clearing station whence he was immediately transferred to a military hospital in England. Owing to the conditions that prevailed at the time, no complete examination was made on admission. The statement on his tally, however, was accepted; a message on an Army Form was sent to his

wife to the effect that he was suffering from a gunshot wound of the right eye, and a railway warrant was enclosed. Shortly afterwards he was dismissed cured, to rejoin his regiment. But he was clever enough to keep the Army Form referred to, and on the strength of it succeeded in imposing upon everyone and evading duty for over a year. His regimental medical officer very naturally accepted the opinion on the Form without of course knowing that it had been given entirely under a misconception. Fourteen months later the man fell into the hands of a specialist, and triumphantly produced the Form. The right eye was said to be greatly impaired by the injury, and the vision was under $\frac{3}{60}$. The left eye read $\frac{6}{36}$. There was no trace of injury, the fundi were normal, and by the interchange of + and − lenses, the vision was brought up to $\frac{6}{9}$ in each with + 1·25 D.

The deliberate upsetting of the trial frame may give a clue to unusual sharpness of sight when rimless lenses are picked up without hesitation from the floor of the dark room. Evidence may be forthcoming whence it is least expected. A man called up under the Derby scheme was sent from the recruiting office to a specialist for a report. He stated that he could only read $\frac{3}{60}$, and in order to do this resorted to various bodily contortions. The fundi and media were normal, there was no refractive error, but tests with trial lenses failed. As the medical officer sat down to make his notes, certain that he had a malingerer to deal with but without a clue, the man, thinking that the examination was over, threw open his coat and stuck his hands into his trouser pockets. The medical officer caught sight of a number of cheap silver medals hanging from his watch-chain, and casually asked what they meant. The man replied that he had won them in competitions. "What kind?" asked the medical officer. "Quoits competitions," replied the man. "I'm a champion quoits player." "Thank you," said the medical officer. The vision was returned as normal.

Sent from the same recruiting office another presented himself one Sunday morning. There was nothing suggestive about him of malingering. He wore a neat civilian suit of black, black bowler hat, white shirt and collar, black tie. He said he was a bricklayer, an occupation which somehow did not agree with his get-up. Still less did his vision of $\frac{6}{60}$ in each. From this he refused to budge. During the ophthalmoscopic examination, which revealed nothing abnormal, he was told that he could see quite well and would be detained until he passed the test, even if had to be kept till ten at night. He was put back and examined a couple of hours later, when he read $\frac{6}{9}$ with ease. Then came the expected whine about three brothers in the Army, his small family, his loss of work, ending with, "and me going to bury my poor old mother this afternoon." So all, down to the suit of clothes, was satisfactorily cleared up.

At an ophthalmic centre there were two medical officers at work, A., a civil surgeon, and B., a commissioned officer. B. was washing his hands in a bunk whose window overlooked a plot of grass where the men to be examined were gathered. A. was finishing the test of a man, who, when

it was over, left the building, jumped down the steps, and burst out laughing when he reached the others. This was not lost on B., who looked at the man's record on the card. It was : "Right $\frac{6}{24}$, Left $\frac{6}{24}$. Refuses any correction. Fundi clear." B. remarked, too, that it was scarcely sufficient. A. agreed, and suggested that the man should be re-examined. After a few more men had been seen, the man in question was called up. He declared boldly and resentfully that he had passed the doctor. B. quietly said, "You haven't passed *me*." The result of that incautious laugh was, "Vision normal. N.A.D."

A perfunctory test with trial lenses may lend an air of malingering in the case of a man who is doing his best to see. Instances of this must have come to the notice of every ophthalmic surgeon. One only will be mentioned.

A man aged 38 was in from a Dispersal Board for a report on his vision. He brought his B. 178 on which was entered an apparently complete statement of his visual defect. "V.R. = $\frac{6}{36}$; L. = $\frac{6}{36}$.

$$\text{R}\ \frac{\mid +1}{\mid}\,{+1} \qquad \text{L}\ \frac{\mid +1}{\mid}\,{+1}$$

"Old macular choroiditis in each. Glasses would not be of the slightest use." This was stated to have been the condition ten weeks before the request of the Dispersal Board. It was found in the first place that the man was myopic. Second, that the macular choroiditis, if ever present, had miraculously disappeared, and the fundi, after a searching examination under homatropin, were normal. Further, with -2 D. in each he read $\frac{6}{6}$.

This case is instructive. Had the earlier report been accepted, the man on discharge would have claimed his pension, probably on the ground of causation, certainly on the ground of aggravation, and he might thereby have been tempted to trade on his defect.

On many occasions men's papers were marked "malingering" when there was the clearest external evidence, such as nebulæ, or a high degree of myopia, to account for their visual disability. From one unit alone in the early months of the war, nine men were sent up as presumed malingerers whose vision ranged from $\frac{5}{60}$ to less than $\frac{1}{60}$, with myopia or myopic astigmatism from $-4{\cdot}5$ to -8 D., and some tact was necessary to convince the commanding officer that these measurements could be made independent of the men's statements.

The concealment of facts is not confined to the soldier : it may extend to his wife. A certain officer of high rank asked a medical officer to examine his wife's eyes as she had difficulty in sewing at night, and possibly was in need of spectacles. Her distant vision was normal ; fundi normal ; no refractive error with the ophthalmoscope. While being examined in the dark room she was casually asked what her age was. She gave it as 42. During the test with trial lenses her face was closely studied, and particularly her neck and hands. She was presbyopic, and from the amount of correction that she accepted the conclusion was that

her age was 52, and she was prescribed for on that assumption. After a consultation of "Who's Who," the startling fact was discovered that if her alleged age of 42 was correct, she must have been married when 11 years of age, and if so, her husband ought to have "done time."

The "pictures," while valuable for detective work, may be just a little overdone. One morning an urgent telephone message came through that a certain George at a V.A.D. auxiliary hospital had "bad eyes," and could he be seen to at once, please? "Bring him at once," was the reply. The medical officer had visions of acute glaucoma. Some time in the afternoon an ambulance arrived, and therefrom alighted a breathless old lady in V.A.D. uniform, plentifully besprinkled with every conceivable or inconceivable Red Cross badge, the "dressing" of which conformed with no known rule in the book. She said she had come forty miles in the ambulance (shortage of petrol), and George was produced. George, boot and lamp cleaner, handy man in the garden, always absent "on an errand for me to the village," when the inspector arrived—George, it appeared, wanted glasses in order to see the "pictures," and his vision of each eye separately was $\frac{6}{18}$ + 1 combined with −1 D. = $\frac{6}{6}$.

"Take him away," said the medical officer.

"But are you not . . .?" exclaimed the lady.

"Take him away," repeated the medical officer, and turned to matters more important.

The net result was petrol wasted for a run of eighty miles. George proved a malingerer, and was ordered to report to his depot without delay.

VII.—The Use and Abuse of Spectacles.

The issue of spectacles to troops was by no means an unmixed blessing to those concerned. A large number of men did not make the best, if any, use of them, and the specialist felt that time was often spent to no purpose.

The Army Spectacle Depot came into existence about February, 1916. Previous to that date, in one district at least, all that the ophthalmologist had at his disposal were empty spectacle frames and loose lenses, ranging from + or − 1 to 4 D.

When at length the Army Spectacle Depot appeared, things improved. The system was elaborate—sometimes there were twenty or more particulars to be entered on each prescription—but it was easy to follow. There were, however, grave disadvantages owing to the limitation of strength of the lenses. In a War Office letter, dated February 2, 1916 (24/G.N./3999, A.M.D. 3), the following restrictions were laid down:—

> The maximum strength of glasses to be supplied will be spheres, 6 D.; cylinders, 4 D.
>
> Quarter-strength glasses will be supplied between 1 D. and 3 D. only, and half-strength glasses above 3 D. up to the maximum referred to.

This seriously handicapped the ophthalmologist and laid on his shoulders the disagreeable burden of explaining to one soldier why he could not get his glasses, while his mate was supplied without hesitation. It also put the specialist in an invidious position. He well knew that if he made a mistake in a correction—excluding presbyopia—the prescription and the spectacles would proclaim it to every specialist whom the patient consulted, and would be a concrete witness to his inefficiency.

Constantly there were grievances which a conscientious medical officer was at his wits' end to settle. The average soldier does not understand compromise. At the same time he does not understand refraction. But seeing his mate provided, let us say, with – 2·5 sph. combined with – 1 cyl. he is furious when told that his correction, which brought his sight up to normal, is outside the limit and that he must go without spectacles unless he buys them himself. At once his sense of injustice is roused, and a willing man is converted into a "grouser."

From time to time "strict compliance with the spectacle scheme" was enjoined, but it was only in December, 1916, that the limits were withdrawn and the strength allowed to be exceeded, "to any reasonable extent." By this date, and much earlier, men who had been operated upon for traumatic cataract were ready for their glasses.

Equipment from the Army Spectacle Depot began to dribble in by the end of February, 1916, and ophthalmic centres were in working order by March. Much of the material came from America, and a word must be said about the trial lenses. Unlike ours and the French, the rims were not coloured (gilt and silvered) to show which were plus or minus. Some genius had decided that the best place for marking the denomination of the lens was the handle, where it was hidden by the finger and thumb. The cylinders had handles, numbered in the same fashion, and their axis was engraved so faintly that mistakes were frequent. The trial-frame was of the time-wasting pattern, in which the lenses were slipped in from below. For retinoscopy in the dark room it was useless, for with the patient's face in shadow, one could never be sure of finding the cell for the lens by touch alone. The refraction equipment was designed to double or treble the amount of time taken over a case, and there was nothing for it but to mark every glass in "plain figures" with a diamond, scrap the trial frame and use one's own.

By May, 1916, the Army Medical Department became conscious that a great deal of wastage was going on in the issue of spectacles, and embodied its views in a letter (May 15), which may be summarized. It pointed out that in many cases glasses were ordered quite unnecessarily, that in the present war of bombs and hand-grenades a high standard of marksmanship in every individual was not essential, that with gunners shooting was done by map and telephone. It went on, "a soldier will not wear glasses unless the benefit which accrues from their wear is very obvious and apparent to himself. In a large number of cases complaining of their sight (quite forty per cent of those tested at the ophthalmic

centres at the front in France), it has been found that glasses are quite unnecessary. It should, however, be remembered that many soldiers of the New Armies have become so accustomed to use glasses in civil life that they are at a very serious disadvantage without them." Finally, attention was called to the difficulty that a soldier had in keeping his glasses clean in the mud of the trenches, that they thereby became useless and the men would not wear them.

Four weeks later the material points of the letters were repeated. At this time nothing was said about gas masks, which at first could not be made to fit closely owing to the sides of the spectacles. This defect eventually was overcome.

It is unnecessary to go into details as to the various changes in the strength of the lenses, which enabled the ophthalmologist to recover his self-respect. It is more important to consider the question of how far the issue of glasses went towards rendering a man more efficient or raising him to a higher category. With regard to the latter, the A.C.I. 690 of April 27, 1917, may be quoted, para 2: "Immediate steps should be taken in all units to ensure that every man classified lower than category A, on account of defective eyesight, is reported on by an ophthalmic expert and provided with suitable spectacles if thereby he can be rendered fit for category A."

A record accordingly was kept to see how far this influenced the situation. In eleven weeks 723 *new* cases were examined—not all refractions. Of these, forty-five went up from B to A, and ten went down from from A to B. Those who went down had nebulæ, strabismus, lenticular opacities and so forth, but they were able to reach the old standard of R. $\frac{6}{24}$, L. $\frac{6}{60}$, no matter what the refractive error was. They could not reach the new standard for category A, laid down in A.C.I. 211 of February 4, 1917, which was $\frac{6}{24}$ in one eye without glasses, and the right eye brought up to $\frac{6}{12}$ with glasses. In one ophthalmic centre, at least, the result scarcely justified the trouble entailed in carrying out the Instruction.

Although the issue of spectacles was satisfactory from the point of view of the consulting room, it was difficult to ascertain whether they were used. In a garrison town, swarming with soldiers on Sunday afternoons, singularly few were seen wearing army spectacles. On one occasion a brigade was met, returning from field exercises. With its strength estimated at the time at less than 3,000 it was interesting to count the men who had on army glasses. There were *four*. This of course does not imply that many others did not possess them: on the march spectacles were not of urgent utility, but the inference is that only four benefited by them to the extent that they wore them constantly. It may have been accounted for by the well-known prejudice among certain classes against spectacles, the idea being that they are a sign of age, and that a man who wears them will lose his job. Whether the war has had anything to do with it or not, it is the case that far more young people

that one sees in the streets and trains are wearing spectacles than did so six years ago.

When a man's vision is less than $\frac{6}{12}$ and is brought up to normal with the appropriate correction, he discovers that he has a visual defect which can be greatly improved. If he is intelligent there is no risk in conveying this information to him, but with the knowledge thus gained he may make himself a nuisance to every specialist whom he encounters. Thus the test with glasses may defeat its own end. Let us take a case, not wholly imaginary. A myopic recruit whose vision is $\frac{6}{60}$ is minutely examined by specialist A. at P. The result with spectacles is $\frac{6}{6}$ and the glasses are issued. The recruit shortly afterwards is transferred to another unit and to another station Q., without his papers. He carefully preserves his glasses which enable him to see "the pictures," but does not wear them. He fails to recognize an officer, is reported, cautioned, and sent to specialist B.

At this stage the recruit has gained some valuable information. He knows that he has a visual defect and that with glasses he can see perfectly, but finding life in the Army not quite up to his expectations he proposes to make the most of his bad sight. He says he has never had glasses, and specialist B., overwhelmed with refraction cases, does his best at one sitting, contenting himself with getting $\frac{6}{12}$, and orders glasses accordingly.

The recruit has added to his store of knowledge. He has one good pair of glasses and another fairly good, he finds that one specialist is extremely careful and that another can be hurried. He is next sent, this time with his papers, to camp for musketry, says he can't see the target with his glasses, and is bundled off to specialist C. This person, having chiefly musketry cases to deal with, has gone to the range and made himself artificially myopic in order to determine how much a myope with a given amount can see without glasses. He has also got himself initiated into the forms and ceremonies observed in the whole art of shooting. The recruit is now faced by a specialist who combines the urbanity of Harley Street with the subtlety of Scotland Yard. His papers are all correct, with a copy of the prescription signed by B., but he thinks he had better not see quite so well as he did, and manages only $\frac{6}{18}$. Specialist C., knowing how B. is situated, leaves the question of glasses on one side for the moment, and talks to the recruit about his first days in the Army, discovers that he had been stationed at P., where he himself once was, and proceeds by casual questions and an indifferent manner to suggest, but never to mention its purpose, the eye department at the barracks. The recruit lets slip the information that on the wall at the foot of the stairs someone had drawn in white chalk a huge human eye. Then C. says suddenly, "What did you see at the pictures last week?" The recruit tells him, and after a few remarks which have nothing to do with spectacles, C. remarks as an afterthought, "About your glasses—there's a little thing the matter with them, so you had better leave them and come back in a week." As there is only one cinema in the place, it is easy to tell off a corporal to watch him there.

The point is often overlooked that in many instances men have to be educated in the use of their glasses. A man who has gone about the world all his life with an uncorrected error has to become accustomed to his spectacles, and if they do not immediately respond to his needs, he is apt to become impatient and throw the things aside. He has, in fact, to uneducate his eyes first of all, and then re-educate them.

In consequence of musketry cases an attempt was made in one district to re-educate men with a rifle as part of the ophthalmic equipment. Musketry instructors seemed inclined to take the view that once a man was supplied with spectacles he ought to be able to shoot with them. With his spectacles on, the soldier was told to aim at the test-types over the sights and read them. It was found that when the correction was above plus or minus 3 D., or even not so much, and more especially when a cylinder was a component, the vision when tested by the rifle with spectacles, was always two or more lines worse than with the direct gaze with spectacles. Some men, further, said that their glasses were of no use for shooting in the prone position. It was rare to find that a man who could read $\frac{6}{6}$ with his correction could make out the same line over the sights, except when the correction was a low one, and then his unaided vision was sufficient for musketry up to 200 yards. Thus the man whose spectacles were of such a power that they did not affect accurate shooting was better without them. With a high power, on the other hand, they were embarrassing, for although he could see the target with the direct gaze, everything was blurred when he took a sight, and he blamed the specialist.

The explanation of the failure of spectacles to improve shooting is simple. The man does not look through the optical centre of his spectacle lens when sighting, but obliquely through the upper and inner segment. In taking aim he rotates his right eye inwards while the spectacle lens remains fixed. He cannot adjust his head so that he can look through the optical axis of his spectacle lens, and the further and more obliquely the gaze is from this point, the greater the distortion. When a cylinder is a component, the blurring is still more pronounced. Periscopic lenses might meet the case in some, but decentred glasses for shooting only would be out of the question.

It is worth pointing out that at one time the best shot in the American Army, an officer, had only $\frac{3}{60}$ with his right eye, corrected with $-2{\cdot}25$ to $\frac{6}{6}$, but it is not stated if a specially decentred lens was used.

The question of shooting with glasses—shooting as part of military training, that is, and not match-shooting—is full of difficulties and apparent contradictions, and if many men with normal vision make poor shots, and if others with poor vision can pass their musketry, some account must be taken of intelligence, attention, and the muscular balance and "sense" in bringing the rifle to the shoulder. Long practice with sporting rifles develops an instinct for aiming at a bird in its flight, and in match-shooting the various allowances that have to be made are often "felt" after incessant practice.

During the Bisley meeting of 1920, a journalist remarked that a large number of the competitors were wearing spectacles, and asked if this was not due to the authorities having given great attention to the eyesight during the war. It might equally have been due to the use of decentred lenses so as to overcome the distortion that has been alluded to, and to the employment of aperture or "peep" sights worn in a spectacle frame. The aperture sight has an effect similar to the pin-hole diaphagm of a photographic camera, by cutting off the circles of diffusion and sharpening the image on the ground-glass plate of the camera or retina. Thus hypermetropes and moderate myopes using a peep sight see the object clearly without glasses. For example, a man's vision is R. $\frac{6}{60}$ hazily; $+ 3$ D. $= \frac{6}{5}$. L. $\frac{6}{180}$ hazily; $+ 5 = \frac{6}{5}$. But with peep sight and no correction, R. $= \frac{6}{9}$.

It would be of advantage if the peep sight, which at present is provided for ranges from 1,600 to 2,800 yards, could be adapted to service rifles for the shortest ranges. Several patterns are in existence, fitted both to match and sporting rifles, but for service conditions they are not officially recognized.

While vision for shooting is of the utmost importance in a standing army, there are many branches of the service outside the combatant ranks, in which a man with appropriate spectacles can be actively employed. Taking the men of the New Armies all round, the number of those visually defective was surprisingly low. In one Division, examined before it went over-seas, the eye cases of all kinds, including external diseases, amounted to a little over four per cent.

It would be worth consideration to codify on broader lines the present standards, so that men fit and willing for particular grades of military service should not be lost to the Army and the Country. And as many ophthalmologists have seen service over-seas and at home, and thereby have made themselves familiar with a diversity of military duties, it would be well to draw upon their knowledge and experience, and enlist their co-operation in all that concerns vision. Neglect to do so must inevitably lead once more to the difficulties which forced themselves upon the attention in 1914-15, and had to be met by the creation of an organization of dimensions which none could have anticipated or dreamt of in times of peace.

One matter may appropriately be mentioned here. Early in the war, the then headmaster of a great public school wrote to the press complaining of the numbers of "fine young fellows" who were rejected on account of defective eyesight and teeth, and with not quite incomprehensible density blamed the authorities for being too strict in this respect. It did not occur to him that he himself, morally responsible for the welfare of the school, that the boys' tutors and the parents, if at all anxious that these "fine young fellows" should be fit for service, should have taken steps beforehand that no physical defect stood in the way of their passing the army standard for officers. After all, consultations with ophthalmologists and dentists were not beyond their financial resources.

It so fell out that the writer was detailed for duty as ophthalmologist on a Medical Board which visited this school to examine candidates for Woolwich and Sandhurst. The proportion of myopes *who had never worn spectacles* was one in ten. These failed to reach the minimum standard without spectacles, and would have been rejected on account of their defect had not the writer estimated the approximate error with the ophthalmoscope and brought their vision up with a correction that was sufficient for the purpose.

In the course of conversation with one of these myopes, whose father's exalted social and political position presumably implied some common sense, the question was put to him how with so great a handicap he could take part in games or sport, or enjoy the theatre or the cinema. Among other things, he said that he sometimes went out shooting with his father. "You'll not go out shooting with me, my lad," was the dry comment.

The issue of spectacles brought into the field a certain class best described as "refraction fiends." Fortunately they were rare. Not skilled ophthalmoscopists or clinicians, but trained as refractionists, on a plane little higher than the commercial optician, they met the case by ordering spectacles in and out of season. It was a constant experience to meet with men who said that the spectacles supplied were of no use, and it needed no searching examination to discover the reason in an obvious pathological condition. The very fact that facilities existed for the provision of spectacles led to abuse of them, and while it was right and just to help a man who therewith could be, and was willing to be, made more efficient, it was undoubtedly the case that too often the prescribing of them was the refuge of many civilians, destitute of all knowledge of military requirements, or of the point of view of the soldier, quite apart from ophthalmological training.

Hospital cases, of course, were given every assistance, but the practice could be overdone. For one man, a cot case, three prescriptions were sent in, one for distance, one for near, and one for playing the trombone. Another young man, sent up by his uncle, who ought to have known better, wanted spectacles for billiards. He was told to bring a billiard table with him next time he came.

It must be admitted in all fairness that the unexpectedness of war found us unprepared in the department of ophthalmology. It has already been said that the demand for man power forced us to see through the eyes of our Allies as to what should be accepted in the way of sight. But the traffic in spectacles would have been reduced by the elimination in the recruiting office of thousands of visually defectives, instead of drafting them into the Army where their inefficiency absorbed a great deal of the time and attention of commanding officers and medical officers who had more serious affairs to occupy their minds.

To decide how a visually defective could best be employed in the Army was by no means the least of the many minor problems which the authorities had to solve.

THE PRACTICAL PREVENTION OF TYPHUS FEVER AND RELAPSING FEVER IN MESOPOTAMIA DURING THE WAR.

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(*Concluded from p.* 61.)

A list of the kit of a British rank and a Sepoy with the shrinkage that occurred is given in Tables II and III, the measurements being taken along the seams. It will be noticed that though the amount of shrinkage is very small after the first immersion, it is negligible after a second immersion, should a corps require a second disinfection. In this connexion it should be remembered that in the manufacture of khaki cloth and army blankets, one of the processes is "shrinking" in which the cloth is immersed in boiling water for four and a half hours. We may assume, therefore, that the shrinkage that occurs by this method is unimportant, even in pure woollen clothing, whilst the large amount of cotton in the khaki

TABLE II.

First immersion for one minute and a half :—

	Original measurement	Measurement after immersion for one minute in boiling water
Socks, woollen—		
1	1 ft. 8 in. ..	1 ft. 8 in.
2	1 ,, 8½ ,, ..	1 ,, 8 ,,
Shirt khaki (inferior flannel)—		
Length, back	2 ,, 11 ,, ..	2 ,, 11 ,,
Sleeve, right, inside seam	1 ,, 6½ ,, ..	1 ,, 6¾ ,,
,, left, ,, ,, ...	1 ,, 6½ ,, ..	2 ,, 6¾ ,,
Trousers, serge khaki—		
Waist, outside circ.	3 ,, 3¾ ,, ..	3 ,, 3 ,,
Left leg, outside seams	3 ,, 3½ ,, ..	3 ,, 2 ,,
,, ,, inside ,, ...	2 ,, 2¼ ,, ..	2 ,, 2½ ,,
Right leg, inside ,, ...	2 ,, 1¾ ,, ..	2 ,, 1¾ ,,
,, ,, outside ,, ...	3 ,, 2¼ ,, ..	3 ,, 2 ,,
Underpants, flannel, white—		
Waist, outside circ.	3 ,, 3 ,, ..	3 ,, 1½ ,,
Right leg, outside seam	1 ,, 10½ ,, ..	1 ,, 10 ,,
Left ,, inside ,, ...	1 ,, 1½ ,, ..	1 ,, 1 ,,
British warm, rough cloth, khaki—		
Back seam, outside	3 ,, 11½ ,, ..	3 ,, 11½ ,,
Left sleeve, outside seam	2 ,, 0 ,, ..	1 ,, 10¾ ,,
,, ,, inside ,, ...	1 ,, 7½ ,, ..	1 ,, 7¼ ,,
Right ,, outside ,, ...	1 ,, 11¼ ,, ..	1 ,, 11¼ ,,
,, ,, inside ,, ...	1 ,, 7 ,, ..	1 ,, 7 ,,
Blanket cloth, smooth—		
Breadth	5 ,, 3¼ ,, ..	5 ,, 3¼ ,,
Length	7 ,, 9½ ,, ..	7 ,, 8½ ,,

TABLE III.

On a second immersion for one minute in boiling water, the following articles showed a shrinkage or expansion as follows:—

Towel ..	Length	− ½ in.
	Breadth	− ½ ,,
British warm ..	Back seam	− ¼ ,,
	Left arm, outside seam	− same
	,, ,, inside ,, ...	+ ¼ in.
	Right arm, outside seam	− ¾ ,,
	,, ,, inside ,, ...	+ ¼ ,,
Underpants ..	Left leg, inside	Same
	Right leg, outside	,,
	Waist, outside circ...	,,
Shirt.. ..	Right arm, inside	Same
	Left ,, ,, ...	− ½ in.
	Length of back	− ¾ ,,
Socks—1 ..	Length	Same
,, —2 ..	,, ...	,,
Jersey ..	Length of back	+ ½ in.
	Sleeve, one	+ 1½ ,,
	,, two	+ 1¼ ,,
Trousers ..	Right leg, outside	Same
	,, ,, inside	− 1¼ in.
	Left ,, outside	Same
	,, ,, inside	+ ¼ in.
	Waist circumference	Same
Blanket ..	Length	− 3 in.
	Breadth	− 2½ ,,

+ denotes expansion. − denotes shrinkage.

cloth of the troops, and the cotton and sacking of the coolies, who were chiefly affected by the disease, rendered the objection negligible. The texture of the blankets and khaki cloth was carefully examined before and after and was unaltered. No difficulty is practically found in maintaining the water at a vigorous boil, if the boiling vats are mounted as described below. I adopted an *oil cooker* for the purpose (shown in fig. 2). It was found that if the oil was mixed in the flue with water a very much greater heat was obtained than if pure oil was used, the water serving to break up the oil and allowing it thereby to burn more rapidly. As is shown in the plan, the oil and water were mounted separately in kerosene tins with channels leading on to a small sheet of metal and controlled by taps. Three ordinary iron baths are mounted in a row on sun-dried bricks, made by the unit, over the oil flame as shown in the plan. This was often modified by the substitution of a disused water tank, half a river buoy, or other available metal utensil. When oil was not available a long trench fire of wood was used. No difficulty was experienced in maintaining the water at the boil. It might be mentioned that, by the combination of water and oil, some oil fires were sufficiently hot to turn metal to a white heat. All that is required is two kerosene tins, two channels and taps, a metal funnel, and a supply of sun-dried bricks or stones.

With reference to the *drying*, the average length of time for drying for various garments, as found in practice, is given in Table IV. Thus it

TABLE IV.—Articles with Respective Times for Drying in Warm Sunshine and Fresh Wind.

	Hr.	Min.
Underpants	2	40
Trousers	2	50
Socks	2	35
Jersey..	5	20
Shirt	2	—
Blanket cloth	2	25
British warm	5	35
Blanket, woollen	1	50
Towel	1	45

will be seen that on a day of medium temperature and wind a man's blankets were dry in less than one and a half hours after immersion and his whole kit, including his British warm, five to six hours after immersion. This is in the case of the heaviest woollen winter kit of the British rank. The enormous majority of kit from natives, coolies, and Indian soldiers

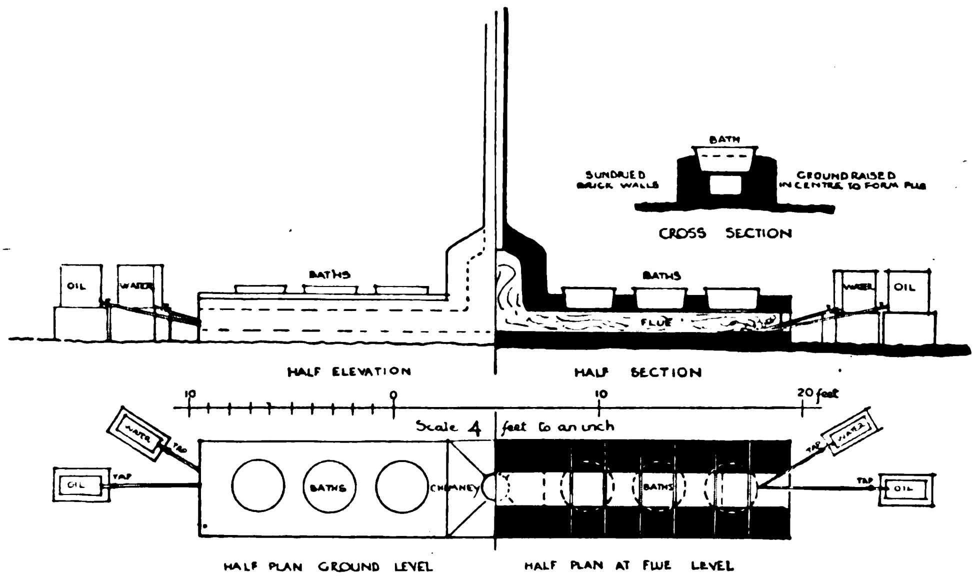

Fig. 2.—Boiling vats (for disinfection of clothes) supplied to all units in Magil Area, units on the Euphrates Defences, etc. First used at Base Isolation Hospital, Mesopotamia.

either was cotton or contained even when woollen, such a large percentage of cotton as to dry very readily, and it was amongst these particularly that lice-borne disease occurred. In one or two cases, when an urgent outbreak demanded immediate delousing on a wet day, a series of *E.P. tents* were run up in a line and connected together. A large number of *charcoal braziers made from kerosene tins,* by puncturing holes in the sides, were placed in holes in the ground inside the tents, with a bamboo fire-guard round each, and the clothes, disinfected as above, were dried on ropes stretched across the tents. This proved very satisfactory in practice.

A barbed wire maze is first erected temporarily as shown in the plan (fig. 1) consisting of a parade ground A, a boiling area B, a lane C, a shaving and bathing area D, and a parade ground E. In the area B a double oil heater was erected, heating six baths of water, in area D were ten baths of Major Christopher's cresol-paraffin-soap emulsion (1 in 30).[1]

The unit or native community enters the parade ground A and falls in with kit and blankets, the gates into A being then closed and a guard stationed. A serjeant goes through the camp or huts to see that no clothing is left behind and to collect all stragglers. Each man strips and takes his kit, tied together and marked with his identity disk, over one arm, and his blankets over the other and passes out (in a group of twenty-five) along the lane C. As he passes down the lane he drops his blankets into one vat and his kit into another. He himself goes forward to the shaving and bathing area D. Each kit is allowed to remain in the boiling water for two minutes and is then pitch-forked from the boiling water on to sheets of reed matting in the area E. When caste allows, the men are clipped. Each man bathes for three minutes in a 1 in 30 solution of Major Christopher's emulsion, a British non-commissioned officer standing by the baths to see that every man applies the solution thoroughly to every part of his body. The men then get out of the baths and are served out with two dry blankets (sterilized the day before) and proceed to the area E, when they pick up their kit from the reed-mats and are allowed to proceed back to camp. Each man dries his kit himself in camp. During the prevalence of relapsing fever and typhus, as many as 10,000 to 12,000 men were dealt with on a Sunday in various units, in one sanitary area, and no difficulty was experienced (up to 2,000 per day being done in certain camps).

The whole apparatus could be readily *mobilized*. Two oxen *transport* carts or a single light motor lorry sufficed to convey the barbed wire, iron pins, baths and cresol solution. Thus one had a complete mobile disinfecting unit which could readily accompany troops on the march or visit outlying villages or posts. A commanding officer could move readily and be accompanied by an efficient disinfecting unit, which the men of his own unit could use, and which, when so used, under the supervision of a single officer, could be relied upon to absolutely delouse a typhus infected unit.

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[1] Formula for kerosene emulsion is as follows :—

Sunlight soap	..	..	..	8 half cakes = 40 oz.
Water ..	..	..	..	1 gallon
Cresol ..	..	..	..	1 ,,
Kerosene oil	..	..	..	4 gallons

Cut up the soap in slices and dissolve by boiling in the water. Add to the cresol and stir till mixed; then add the oil and stir again. The whole readily mixes to form a dark fluid which can be kept bottled.

Material for mobile disinfecting unit by boiling water estimated for a battalion :—

Zinc iron baths	8
Oil cookers	4
Chimneys	1
Barbed wire	300 yards
Wire pegs.	
Kerosene tins	6
Kerosene soap emulsion.	
Reed mats.	
Hair clippers.	
Oil or wood fuel.	

Table I gives the list of requirements for a large area comprising a population of 18,000 in addition to civilians and including nineteen units. It will be noted that there is no handling of infected clothing by the personnel engaged in disinfecting, the kit being dropped by the owner into the boiling water and being lifted out by a wooden stick or fork. A different vat can be used for each caste, so obviating other possible difficulties.

(*B*) *Troops on the Line of Communication.*—For the disinfection of troops stationed on a railway, the most efficient and economical method is the use of steam, released under pressure, amongst the kit and clothing.

The first attempt with this method in Mesopotamia was made with a vertical boiler which was mounted on a railway truck and which passed steam into a small van behind. The boiler-pressure stood at 100 lb. to the square inch when the experiment commenced, but rapidly fell as shown in Table V.

It became necessary, therefore, to use a boiler in which steam could be generated more rapidly, that is a high pressure maintained in the boiler for half an hour, with steam passing into the vans.

A locomotive (Norvi Railway, No. 16), with two large closed metal vans, constituted the disinfecting unit. The engine was coupled by copper steam pipes, fitted with taps, to both vans, and so arranged that each van could be used independently. The van doors, etc., were fitted with flat rubber so as to ensure close-fitting (see below), and shelves for kit were arranged to allow for free circulation of steam round the kits.

Both vans were filled with kits, one completely, the other partially, and a series of experiments undertaken to determine the efficiency of the method. As shown in Tables V and VI, lice-eggs, fleas, and bacteria in ten folds of blanket were destroyed after twenty minutes steaming. The steam pressure, useful as representing the amount of steam entering the van, is shown in minute intervals during the experiment. It will be noted that an average of 120 lb. to the square inch in the boiler is maintained during the time that steam was entering the van. These figures should be contrasted with the figures given for the stationary boiler, it being remembered that, whilst no attempt was made to maintain a high steam pressure

TABLE V.—RESULTS OF ORIGINAL EXPERIMENTS FOR EFFICIENCY OF LOCOMOTIVE VAN DISINFECTOR.

Van No. 1	containing 379 blankets ..	20 minutes	
Potatoes ..	2, 4, 6, 8, 10 folds ..	cooked	
Hens' eggs ..	8—12 ,, ..	hard boiled	
Lice (live) ..	6 ,, ..	killed	Control 1
..	8 ,, ..	,,	
Eggs 2 folds ..	..	,,	Control 2
4 ,, ..	..	,,	
6 ,, ..	..	,,	
8 ,, ..	..	,,	
10 ,, ..	..	,,	Control 3
Van No. 2	containing 250 blankets		
Potatoes ..	Folds 2, 4, 6, 8, 10	cooked	
Eggs	8	hard boiled	
	12		
Lice eggs ..	2	killed	Control 4
	4		
	6		
	8		
Controls ..	C 1	Lived	
	C 2	Hatched out	
	C 3	,, ,,	
	C 4	,, ,,	

Cholera, typhoid, coli bacilli in 8 folds blanket Killed
Controls lived
Fleas in 10 folds blanket Killed
Controls lived

TABLE VI.—STEAM PRESSURES TAKEN EVERY MINUTE DURING THE ORIGINAL EXPERIMENTS WITH THE LOCOMOTIVE DISINFECTOR IN MESOPOTAMIA. STEAM PRESSURES, 0 PER SQUARE INCH. TAKEN AT BOILER OF ENGINE.

Commenced 10.33 *a.m.*

a m.	Deg. lb. sq. in.		a.m.	Deg. lb. sq. in.	
10.33	115		10.49	115	
34	115		50	95	
35	115		51	95	
36	115		52	95	
37	115		53	100	
38	115		54	105	
39	115		55	105	
40	115	Steam injector opened	56	105	Steam injector opened
41	115		57	105	
42	115		58	110	
43	115		59	115	
44	115		60	118	
45	115		(11 a.m.)		
46	115		11.1	120	
47	115		2	110	
48	115		3	115	

Steam Pressure during Experiments with Original Type of Boiler.

Steam was admitted into the van at 100 lb. pressure from the boiler.

The pressure decreased as under :—

From 100 lb. to 60 lb. during the first five minutes. It then fell 5 lb. every five minutes with the steam cock half open until it reached 40 lb.

After twenty-eight minutes from the commencement of the test when temperature was still standing at 40 lb. per square inch, the steam cock was open full, the pressure then fell to 38 lb. in the last two minutes.

It was very difficult to keep steam in the boiler as the blower had to be in continual use, and with the steam entering the van the boiler could not generate steam sufficiently rapidly to meet the requirements.

The boiler used was a 4 horse power boiler with a pressure of 100 lb. to the square inch.

TABLE VII.

British warm new: 1—		
Breech seam	3 ft. 4¼ in.	3 ft. 4 in.
Bottom	5 ,, 2½ ,,	3 ,, 2½ ,,
Left inside seam	1 ,, 5½ ,,	1 ,, 5¼ ,,
,, outside seam	1 ,, 10¼ ,,	1 ,, 10 ,,
Right sleeve, inside	1 ,, 10¾ ,,	1 ,, 10 ,,
,, ,, outside	1 ,, 5½ ,,	1 ,, 5¼ ,,
British warm new: 2—		
Breech seam	3 ,, 7 ,,	3 ,, 5½ ,,
Bottom	5 ,, 6 ,,	5 ,, 3½ ,,
Left inside seam	1 ,, 6 ,,	1 ,, 5¼ ,,
,, outside seam	1 ,, 10¾ ,,	1 ,, 10¼ ,,
Right inside seam	1 ,, 5½ ,,	1 ,, 5¼ ,,
,, outside seam	1 ,, 11 ,,	1 ,, 11 ,,
Coarse Harden pyjama trousers—		
Right inside	2 ,, 4½ ,,	2 ,, 3 ,,
,, outside	3 ,, 2½ ,,	3 ,, 2 ,,
Left inside	2 ,, 3½ ,,	2 ,, 2 ,,
,, outside	3 ,, 1½ ,,	3 ,, 1½ ,,
Top	3 ,, 3¼ ,,	3 ,, 3 ,,
Coarse Harden pyjama trousers—		
Left outside	3 ,, 3¼ ,,	3 ,, 3¼ ,,
,, inside	3 ,, 1½ ,,	2 ,, 1½ ,,
Right outside	3 ,, 3 ,,	3 ,, 3 ,,
,, inside	2 ,, 1 ,,	2 ,, 1 ,,
Top ,,	3 ,, 2¼ ,,	3 ,, 2 ,,
Woollen football jersey—		
Left inside	1 ,, 8½ ,,	1 ,, 8 ,,
Right ,,	1 ,, 9¼ ,,	1 ,, 8 ,,
,, sleeve middle	1 ,, 11½ ,,	1 ,, 10 ,,
Left ,, ,,	1 ,, 11¾ ,,	1 ,, 10¼ ,,
Internal circumference, bottom	3 ,, 5¼ ,,	3 ,, 3¼ ,,
White flannel undervest: short sleeves—		
Length back seam	2 ,, 4½ ,,	2 ,, 4 ,,
Bottom	4 ,, 2 ,,	4 ,, 2 ,,
Woollen and cotton undervest—		
Back seam	2 ,, 6 ,,	2 ,, 5½ ,,
Bottom	4 ,, 5 ,,	4 ,, 4 ,,
Left sleeve seam	9½ ,,	9 ,,
Right ,, ,,	8¼ ,,	8½ ,,
White flannel underpants—		
Right outside seam	3 ,, 1½ ,,	3 ,, 1½ ,,
,, inside seem	2 ,, 5½ ,,	2 ,, 5½ ,,
Left outside seam	3 ,, 1¼ ,,	3 ,, 1¼ ,,
,, inside seam	2 ,, 6½ ,,	2 ,, 5 ,,
Top outside seam	2 ,, 11¼ ,,	2 ,, 10½ ,,
Flannelette underpants—		
Right outside	3 ,, 3 ,,	3 ,, 3 ,,
,, inside	2 ,, 6½ ,,	2 ,, 5½ ,,
Left outside	2 ,, 11 ,,	2 ,, 10¼ ,,
,, inside	2 ,, 7½ ,,	2 ,, 7 ,,
Top	3 ,, 3½ ,,	3 ,, 2½ ,,
Woollen cardigan jacket—		
Bottom	4 ,, 0 ,,	3 ,, 8 ,,
Left arm	1 ,, 10¼ ,,	1 ,, 8 ,,
Right arm	1 ,, 10 ,,	1 ,, 8 ,,
Comforter, woollen—		
Length	2 ,, 11½ ,,	2 ,, 8¼ ,,
Width	1 ,, 0 ,,	10½ ,,

in the van, the steam pressure, as shown in the gauge, varies directly with the rapidity with which steam is being generated when the tap to the van is open, that is, represents the quantity of steam entering the van. It is the rapidity with which a locomotive boiler can generate steam that makes this type of boiler valuable for disinfection work. The figures further show that the use of the steam injector during the experiment should be forbidden, owing to the consequent fall in boiler-pressure. A further series of experiments were done in order to determine the effect of steam on the shrinkage and texture of clothes. The results are shown in Table VII.

During the practical section of the experiment (at Ur junction), the capacity of the machine for dealing with numbers was tested, lice-eggs being inserted at intervals during the process, to test the efficiency. In five hours 1,063 kits and 2,150 blankets were passed through the disinfector, each article remaining in the van for thirty minutes.

Each van was used alternately in order to waste no time of the personnel. The times for loading and unloading and the actual times for opening and shutting of the vans is given in Table VIII.

TABLE VIII.—Actual Times taken for the Loading, Disinfection and Unloading of the Vans.

Van 1.—

Loaded, 10 a.m. to 10.15 a.m.	100 kits in 1½ hours
Disinfecting, 10.15 a.m. to 10.45 a.m...	
Waiting until van was cool enough to enter, 10.45 a.m. to 11 a.m.	200 blankets
Unloading, 11 a.m. to 11.5 a.m.	
Kits drying and issued to men, 11.5 a.m. to 11.30 a.m.	

Van 2.—

Loaded, 10.15 a.m. to 10.35 a.m.	100 kits
Disinfecting, 10.35 a.m. to 11.5 a.m.	200 blankets in 1¼ hours
Waiting van to cool, 11.5 a.m. to 11.15 a.m.	
Unloading, drying and issuing to men, 11.15 a.m. to 11.30 a.m.	

Blankets were found to be dry fifteen minutes after the van was opened. The outside of the bundles of kit was completely dry in the same time. The inside was very hot and dried within a few minutes of the bundles being opened.

In the event of the engine running continuously day and night, one day in every six is required for oiling and overhauling. If the engine is used for shorter hours the necessary overhauling can be done at the end of each day.

No difficulty was experienced with regard to caste—the clothing being packed separately on different shelves in the van.

The first step in the delousing of a unit is the complete disinfection of personnel working the train, in order to prevent the re-infection of the clothes during handling. This, together with the disinfection of 200 blankets (the use of which will be shown later) and the necessary preparation of baths, steam pressure, etc., is easily completed in an hour.

The personnel then put on a complete overall covering for their protection against typhus-infected lice. This precaution is most important but is one often neglected in typhus fighting. Practically we found that lice readily passed through tightly rolled puttees, down the folds, whilst the ordinary uniform offers an easy means of infection by lice. To protect

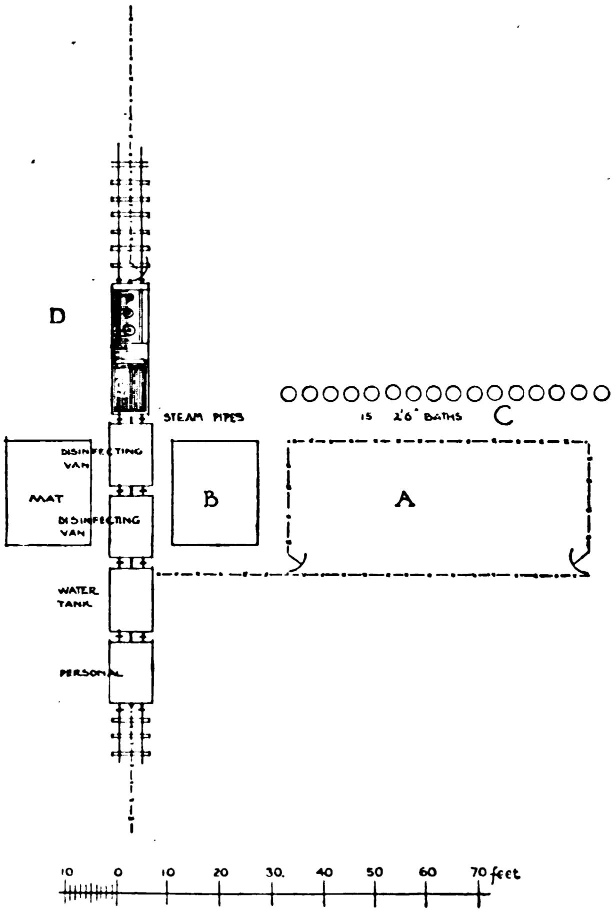

Fig. 3.—Arrangement of barbed wire enclosures for use with disinfecting train. First used at Ur, Mesopotamia.

the body of the personnel, garments were made from sheets, with a cowl to cover the head completely and tying under the chin, with closely fitting wrists, fastening down the back with a large overlapping piece, and with trousers attached so as to form one complete garment. The whole garment was so made that a short length of closely placed buttons down the back

sufficed to completely enclose the wearer, the man stepping into the garment; the trousers were made complete with feet. Gum boots were then put on, the interval between the leg being packed with cotton wool soaked in paraffin or naphthalene. Thick rubber or leather gloves were finally put on and pulled well up over the wrists. No case of either typhus or relapsing fever occurred in the personnel of the train, when so guarded, throughout the time I used the train.

Two large areas, A and D, were staked off on either side of the railway line and a good barbed wire fence run up. Twenty iron baths, C, were filled with 1 in 30 Christopher's solution. The train partially separated the areas A and D, the division being completed by barbed wire (see fig. 3).

TABLE IX.—STAFF AND EQUIPMENT OF NO. 1 DISINFECTING TRAIN, MESOPOTAMIAN EXPEDITIONARY FORCE.

Van 1.—

Baths, 12; reed matting; kerosene soap; emulsion, 60 gallons; firewood, 4 cwt.; kerosene tins, 12; barbed wire, 300 yards; spades, 2; burnt bricks, 200; incinerator bars, 24; hair clippers, pairs, 2.

Van 2.—

Kit of B.O. and B.O.R.s, 3.

Van 3.—

Sweepers, 6 with 6 days' rations and kit.
Carriage, 3 Sanitary B.O.R.s with 6 days' rations.
Carriage, B.O. (Sanitary Officer) with rations and kit.
Brake-van, Indian Sepoy Guard of N.C.O. and 8 men.

—Extract from Section Orders, 29th Sanitary Section, Order No. 54.

The unit to be disinfected fell in by platoons or, in the case of natives, under their head men, with their blankets and kits tied in separate bundles, each man's identification disk being fastened to his kit. A serjeant was then sent round the camp or huts, for clothing left behind and for stragglers, and a guard placed on the entrance to A. Each platoon or group of twenty went up to the train successively and dumped their kit outside the van, on the reed mat, B. The group then proceeded to the baths where under the supervision of a British sanitary non-commissioned officer they bathed themselves completely with the emulsion for three minutes, and then passed through the barbed wire gate in front of the engine to D, where two blankets (from those previously disinfected) were issued to each man, in which he wrapped himself until his kit was unloaded from the van. As each kit was unloaded the number on the identity disk was read out and each man received his kit, retaining the two blankets issued to him, his own being kept for the next party and issued to them during their wait. The staff and equipment of the train is given fully in Table IX. Three British sanitary ranks only are required—one non-commisioned officer supervises the infected kits in area A, loading the vans and turning on the

steam, a second supervises the unloading in area D and the issue of clean blankets and kit, whilst the third is responsible for the preparation of the cresol baths, for their constant change, and for seeing that the men efficiently apply the emulsion to the whole surface of the body. Each non-commissioned officer has a staff of Indian assistants. The change of the baths is effected by having two more baths than are actually in use, two baths being changed successively as each new batch of men arrives.

For troops on the line of communication, and therefore probably adjacent to a railway line, this is undoubtedly the most efficient and the quickest method for large numbers. The results can be definitely relied upon, provided a careful watch is kept on the locomotive steam-pressure gauge and on the constant renewal of the strength of the cresol emulsion in the baths.

Although my work with a disinfecting van and locomotive was completed independently of Colonel Hunter (I did not see his papers till I reached England), these results amply confirm his high opinion of the use of a locomotive for disinfection, when a railway line is available.

I would, however, mention some modifications in the use of the locomotive in Mesopotamia.

In order to make certain that the steam passed completely through the van and did not escape at the doors without completely penetrating all the bundles, the doors were made steam-tight with flat white rubber. All other outlets were closed, except one at the far end of the wagon and on the floor. This is important as otherwise it is impossible to be sure that the steam is not escaping by a short route (e.g., a door fitting) and disinfecting only a section of the wagon. Further, our vans worked alternately, so that the loading and unloading could proceed continuously, one van being open whilst the other was shut. This served the double purpose of enabling us to get twice the quantity of steam into each wagon, and as no time of the personnel was wasted we therefore could do twice as many kits in an hour.

It appeared that one hour's exposure was unnecessary and that with lice-eggs in ten layers of clothing, placed in different parts of the van, no hatching occurred after twenty minutes' exposure, thirty minutes allowing an absolutely safe margin. Thus working the vans alternately and allowing each kit thirty minutes' exposure, 210 kits and 210 blankets could be readily disinfected per hour.

It is insufficient to have a corporal only in charge of the disinfection. If disinfection is to be relied upon a keen sanitary officer with a small guard for ensuring order and discipline is essential—otherwise some men escape delousing, e.g., cooks, etc.

The chief point, however, in which I am not in agreement with Colonel Hunter, is his statement that the disease will stop if the men are disinfected in batches so that the whole unit is disinfected every two or three weeks, a fresh batch being disinfected daily. This may have been satisfactory in the face of a sweeping epidemic such as that of Serbia in

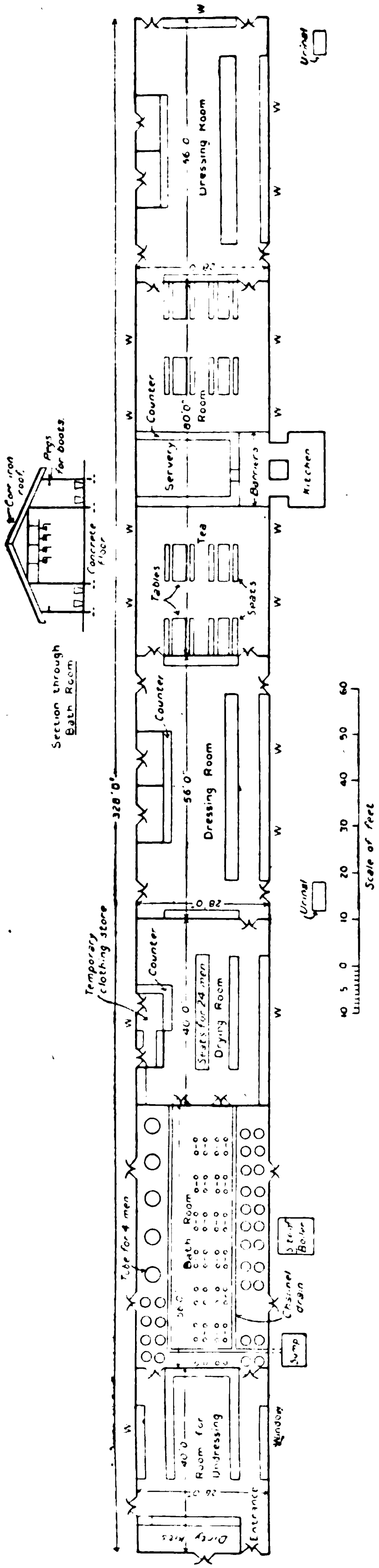

FIG. 4.—Disinfecting Station, No. 9 Berth, Magil.

1915, but it appears to me that the great value both of the railway disinfector and of disinfection by boiling, as described above, lies in the fact that it enables the whole process to be completed in a day and avoids, a proceeding we found most unsatisfactory and ineffective in the prevention of disease, the delousing of a unit in groups of men. It was my experience that in order to stop an outbreak in a unit or group of units, it was essential to parade every single man with his full kit, send a serjeant round the camp for stray clothing and men, and disinfect the whole camp at one sitting. I have found this stopped relapsing fever at once, in units that were doing the sectional delousing every week, and who continued to have a weekly crop of cases of lice-borne disease throughout the winter, until this was done.

(*C*) *Troops at the base.*—This differs from the foregoing in that a more elaborate apparatus is possible and that, as the troops are either proceeding to England for demobilization, or to some other war-area by ship, it is essential that the means of disinfecting three to four battalions a day, independently of the weather, should be available. A building is therefore necessary. Further, as the troops, who have been exposed to plague and typhus infection, may be proceeding to uninfected war areas, the method must be absolutely efficient.

The disinfecting station designed and equipped is shown in fig. 4 as eventually constructed on No. 9 wharf, Basra, and was

the first disinfecting station in Mesopotamia to deal with large numbers of troops.

The actual disinfecting unit was a locomotive and vans running on a rail adjoining the building. Each unit was disinfected in groups of 150 under an officer or non-commissioned officer of the unit.

TABLE X.—ESTIMATE OF PERSONNEL, ETC., FOR DISINFECTING STATION, MAGIL.

Estimated Staff for 2,000 *Men*, 2,000 *Kits and* 4,000 *Blankets.*

1 B.O.	14 sweepers.
9 B.O.R.'s including a serjeant.	10 coolies.
2 I.O.R.'s.	10 other followers.

These will be distributed as follows :—

1 B.O.R. in undressing room.	1 B.O.R. supervising disinfecting vans.
2 B.O.R.'s supervising bathing.	1 B.O.R. in charge of tea.
2 B.O.R.'s issuing clothing in dressing rooms.	1 B.O.R. at exit.

(Of all the B.O.R.'s only two need have had sanitary training.)

2 I.O.R.'s issuing blankets.	10 sweepers to carry clean clothing to dressing rooms.
10 coolies to carry infected clothing to disinfector.	2 bhisties to work at heating arrangements for water.
4 sweepers to load and unload.	8 bhisties for changing baths.

Estimated time for 1,000 with kits and 2,000 blankets, 6 hours.

Each group entered the undressing-room and stripped. Each man tied his clothes, uniform and kit together, and attached his identification disk to the bundle which he passed, with his blankets, on to the counter. The kits were then loaded into the van by the personnel of the station and whilst steam was being passed into the van, the locomotive proceeded down the rails parallel to the station, to the entrance to one of the dressing-rooms, where at the end of half an hour the kits were unloaded and reissued. The group for disinfection in the meantime passed through the swing doors into the bathroom where each man had three minutes in a hot cresol emulsion bath and two minutes under a hot shower (hot water being piped on from the boiler shown in the plan). In the meantime the undressing room was occupied by a fresh batch of 150 men (Group 2). The first batch (Group 1) then proceeded to the drying room, where each man was issued with a pair of pyjamas, a dressing gown and slippers. Group 2 now enters the bathroom and Group 3 is in the undressing room, while Group 1 is in the dressing room where their kits are issued from the counter, a cup of hot tea supplied to each man, and their pyjamas, dressing gowns and slippers collected and returned by the station personnel to the drying-room. As Group 2 leaves the drying room the men pass by a roped-off passage through the first dressing-room (where Group 1 is) to the second dressing-room. Each dressing-room received a batch alternately and worked in conjunction with its own disinfecting van outside. By this means, during the period of demobilization three to four battalions could be passed through the station every twenty-four hours.

The only personnel required was an officer and serjeant in charge,

British ranks for the undressing room 1, each dressing-room 2, bath-room 2, with an Indian or coolie staff for the changing of the cresol emulsion, a cook 1 (for the tea) together with the usual personnel of the disinfecting train—a total of 9 B.O.R.'s and an Indian or coolie labour staff of 30 to 40 men. The staff is given in Table 10.

The station was constructed of corrugated iron with a cement base covered, except in the bathroom, with cocoa-nut matting or duck-boards.

Later, this type of station, with the necessary modifications in the case of Indians, was constructed for Indian troops at No. 1 Indian Base Depot, and for Indian followers at No. 5 Indian Base Depot, each being provided with its own disinfecting train. It was possible, therefore, for all types of troops for demobilization to be efficiently disinfected, preparatory to embarking, at the rate of 3,000 to 4,000 a day.

To summarize, in combating typhus and relapsing fever in a country in which these diseases are not endemic, it is essential that a method of louse-destruction invariably fatal to lice-eggs should be adopted, that it should be effective in untrained hands, and that it should enable the whole unit to be completely disinfected in one day. The use of Thresh machines, Serbian barrels, sulphur chambers, insecticides, ironing, etc., as practically used, do not fulfil these conditions. For advanced troops, outlying military units and for recently occupied villages the author has found the use of boiling water on an extensive scale, combined with rapidly run up barbed wire cages, to be practical and absolutely reliable, and would urge that this is the best method. For troops and natives on the lines of communication (i.e., presumably within marching distance of a railway) the disinfecting train is the most satisfactory unit, combining as it does, reliability with speed of work.

For troops at the base, especially those exposed to possible infection by plague fleas or typhus lice and proceeding to uninfected areas, a carefully organized and supervised disinfecting station is required. The combination of a building for bathing and waiting with a train for disinfecting will be found to be the most reliable method and one which will deal efficiently with the largest number in the shortest time.

In conclusion I wish to thank Lieutenant-Colonel Morris, R.A.M.C., and Lieutenant-Colonel F. E. Fremantle, O.B.E., R.A.M.C., for their invaluable assistance and advice, Captain E. H. Harries, R.A.M.C., whose reliable clinical work rendered early preventive measures in units possible, and Captain K. B. Mackenzie, M.C., A.R.I.B.A., late 7th D.W. W.R.R., for much time spent on the preparation of the photographs, plans, etc.

Clinical and other Notes.

A CASE OF COSCOROBA BACILLURIA.

By Captain T. O. THOMPSON.
Royal Army Medical Corps.

Lieutenant M. appeared before a medical board after demobilization for continued illness after malaria, and was admitted into hospital for observation and treatment.

History.—He had done eleven years' planting in very malarious areas before the war without once getting malaria. During mobilized service in Mesopotamia and France he developed a chronic condition of pain in the kidney regions, with general malaise and debility.

He was returned to England, where he was treated with a long course of vaccine, stated to be an autogenous *Bacillus coli* vaccine for bacilluria. Incidentally during this time he had been diagnosed as a case of colitis, chiefly, apparently, because of abdominal pain and tenderness in the upper left iliac area.

He returned to India for civil employment on tea and rubber estates, and very shortly succumbed to an attack of malaria. This had been continuing for some time when he came before a medical board for pension purposes and was admitted to hospital for observation.

On Admission malaria benign tertian parasites were immediately found in the blood and quinine treatment was commenced. In addition, however, he complained of severe intermittent abdominal pains on the left side of the umbilicus and in the left iliac fossa; for this he stated he had been diagnosed colitis. There was no diarrhœa, and no thickening or tenderness of the colon was apparent; the tongue was furred and the appetite very poor; there was general debility and lethargy. Splenic tenderness was present at first but cleared up with quinine.

The debility and lethargy had been periodic, being more pronounced every five or six weeks for about ten days, and being accompanied by an increase of pain and general discomfort. In spite of quinine treatment the symptoms, including a slight evening rise of temperature, continued while in England.

Treatment with Vaccine.—Bacteriological examination of the urine indicated that there were a large number of organisms, mostly of one kind, present in it.

Therefore a catheter specimen of urine was obtained from the bladder with the utmost aseptic precautions soon after a natural evacuation of urine. This urine was plated, out on Conradi plates in the usual manner for enteric group or other organisms.

A pure culture of a bacillus was obtained, which on further examination was found to be *B. coscoroba*. Sub-cultures were sent to the District Laboratory for confirmation.

Cultures of this were put up on agar and a vaccine prepared from these. The vaccine prepared was of a strength of about 6,200,000 per cubic centimetre.

An initial dose of half a cubic centimetre, i.e., about 3,100,000, was given, as it was not considered that this organism would have any great pathological reaction.

A most violent reaction occurred within six hours, which took four days to clear up. The chief points of the reaction were:—

British ranks for the undressing room 1, each dressing-room 2, bathroom 2, with an Indian or coolie staff for the changing of the cresol emulsion, a cook 1 (for the tea) together with the usual personnel of the disinfecting train—a total of 9 B.O.R.'s and an Indian or coolie labour staff of 30 to 40 men. The staff is given in Table 10.

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An initial dose of half a cubic centimetre, i.e., about 3,100,000, was given, as it was not considered that this organism would have any great pathological reaction.

A most violent reaction occurred within six hours, which took four days to clear up. The chief points of the reaction were :—

(1) A temperature up to 105° F.

(2) Intense pain in the lumbar regions and tenderness all round the kidneys on both sides.

(3) Marked irritability of the bladder, i.e., micturition every few minutes with the passage of only a few drops of urine and great pain and scalding during the act.

As soon as the temperature dropped the patient felt very much better except for the local pains; these continued, decreasing for six to seven days. A second dose was given one week after the disappearance of the pains. This time the dose was reduced to 400,000. A very satisfactory slight reaction was obtained, the chief point again being slight pain round the kidneys and slight irritability of the bladder.

The next three doses all had the same slight but decreasing effect, the dose being increased to half as much again on each occasion.

A very marked improvement set in. The patient became robust and with a good appetite began to put on fat. He stated that he had not felt so well for several years. He also stated that the *B. coli* vaccine had never had any marked effect on him and had never given him any such benefit.

He had to return to England before his course was completed and took the remaining doses with him. He considered that he had been cured of his chronic complaint.

Comments.—It may be presumed that probably his condition has been a coscoroba bacilluria from the beginning and that a *B. coli* vaccine should not have been used. There seems no doubt that *B. coscoroba* was the actual causative organism since it was obtained in pure culture from catheter urine and also from the fact that it had such marked results.

The very marked reaction to a small dose of an organism which one would not naturally regard as pathogenic was somewhat startling. The dosage should presumably commence with not more than about 200,000 and rise by about 200,000 for eight to ten doses.

The chief feature of the case, however, is the nature of the causative organism and the satisfactory results of treating the same with an autogenous vaccine. In all the literature available (Pasteur Institute Library available by kind permission of Lieutenant-Colonel Cornwall, I.M.S.) no mention or trace of a case of coscoroba bacilluria could be found. It is therefore considered that this case has sufficient interest to bring it to the notice of others who may chance to meet a similar condition, especially as regards treatment with autogenous vaccine.

A CASE OF SPLENIC ANÆMIA IN A CHILD DUE TO *LEISHMANIA INFANTUM*.

By Major T. E. H. GATT.

Royal Army Medical Corps.

A male child, aged 3, was admitted to the Military Families Hospital, Curragh, late in August, 1920.

History.—The patient was born in Malta, and lived on the island until 2 years old, when it was brought home by the parents apparently in good health. While in Malta it had associated with a poodle from time to time.

State on Admission.—Medical advice was not sought by the parents until May, when they were alarmed by the increasing pallor and protuberant abdomen. The child was first seen by Dr. Agnes Murphy, who referred to me for a general examination of the blood, with a view to determine, if possible, the cause of the anæmia. The blood finding was as follows: Blood smear, extremely watery, almost invisible; hæmoglobin (Gowers' hæmometer) about fifty per cent; total white count 7,000.

Differential count:—

Polynuclears	37
Mononuclears	38
Large mononuclears	25
Others	—
	100

Parasites.—*Leishmania infantum* bodies in large endothelial cells (twenty counted in one cell), in large mononuclears, and free.

The reds were not appreciably altered.

The abdomen was very large, and the spleen, which was hard and with a well-defined notch, was largely responsible for the enlargement.

The parents were advised to bring the patient to hospital for admission, but they did not consent until later, when the child developed broncho-pneumonia.[1]

Treatment and Progress.—As soon as possible after the diagnosis was established, the patient was treated with organic antimony, the compound used being acetyl-P-aminophenyl stibiate of sodium as prepared by Messrs. Allen and Hanburys under the name of stibenyl, given intramuscularly in progressive doses, beginning with 0·05 gramme.

After the fourth dose the splenic enlargement showed appreciable reduction, and the death-like pallor also improved somewhat. The patient, however, succumbed to the complication above referred to within a week of its development.

A NOTE ON FOUR CASES OF INFECTION WITH *BACILLUS FÆCALIS ALKALIGENES*.

By Major O. P. Watson.
Royal Army Medical Corps.

Although *Bacillus fæcalis alkaligenes* is stated to be usually non-pathogenic, disease due to this organism is recognized and is fully described in Castellani and Chalmers' "Manual of Tropical Medicine," and the following note is written because in two, at any rate, of the cases under notice, bacteriological and serological findings give rise to the suspicion that *B. fæcalis alkaligenes* may not have been

[1] As other instances of secondary infections in leishmaniasis have come to my notice I should like to quote here my experience of the same disease in Northern India among a few European soldiers similarly affected.

In these cases (about eleven patients in all) the most common infection was *Bacillus pyocyaneus*, apparently of intestinal origin. This organism was isolated from blood and urine. One case was so markedly similar to enteric fever that the patient actually found his way to an enteric convalescent depot.

Pyocyaneus infection is very common in some parts of North India and certainly esponsible for septicæmias and a form of dysentery.

the true cause of the disease, although isolated from the blood: and it seems desirable that full investigation should be made in other cases of this infection.

The cases under notice were three British soldiers and one Jemadar of the Indian Army. From the blood of all four men *B. fæcalis alkaligenes* was isolated in the Mhow Divisional Laboratory and subsequently confirmed at the E.F.C. Depot, Naini Tal.

Two of the British soldiers were stationed at Jhansi and the third apparently contracted the disease at Nagpur. The Jemadar was stationed at Mhow.

CASE 1 (BRITISH SOLDIER).

Clinically the case belonged typically to the enteric group. There was continued pyrexia for eighteen days, reaching 104° F. during the first week, then becoming remittent in type and falling by lysis, followed by a typical relapse lasting three weeks in which the temperature ran somewhat lower than the first bout. Abdominal distension and rose spots were present.

At the E.F.C. Depot his serum agglutinated *B. paratyphosus* "A" in a dilution of 1 in 2,000. Eight months later the agglutination with para "A" had dropped to 1 in 100, the agglutination with *B. fæcalis alkaligenes* being 1 in 20.

CASE 2 (BRITISH SOLDIER).

Suffered from initial pyrexia of fourteen days, falling by lysis and immediately followed by a remitting low pyrexia lasting for fifty-two days and terminating on the aspiration of ten ounces of clear fluid from the chest. In the initial attack there was abdominal discomfort, no rose spots or obvious enlargement of spleen. Benign tertian parasites were found in the blood at the beginning of the attack and again after aspiration of the chest, when there was typical malarial pyrexia with sharp rise on the first and third day.

Agglutination reactions carried out at E.F.C. Depot were as follows :—

	T.	A.	B.	B.F.A.
(1) 4½ months from beginning of disease	500	100	20	40
(2) 5½ ,, ,, ,, ,,	160	80	20	20

CASE 3 (BRITISH SOLDIER).

This case is of great interest as he has eventually been found to be secreting *B. typhosus*, thus strengthening the suspicion, already aroused by the serological reactions of the two preceding cases, that *B. fæcalis alkaligenes*, although isolated from the blood, may not have been the causative agent of the disease.

The temperature chart of this case was one of typical uncomplicated typhoid fever, with continued pyrexia for twenty-three days falling by lysis and followed by apyrexia broken only by one malarial bout, when benign tertian parasites were found in the blood. There is no suggestion in the chart of any superimposition of infection.

The fever was accompanied by diarrhœa, but there were no rose spots or splenic enlargement. A dicrotic pulse was noted and the pulse and temperature ratio was markedly low, e.g., temperature 104° F., pulse 90.

The serological reactions as tested at the E.F.C. Depot are as follows :—

	T.	A.	B.	B.F.A.	Own organism (T.)
(1) 2½ months after beginning of disease	1,000	250	250	—	—
(2) 3½ ,, ,, ,, ,,	500	100	20	20	1,000
(3) 4½ ,, ,, ,, ,,	500	50	20	20	1,000
(4) 5½ ,, ,, ,, ,,	500	50	*Nil*	—	500

He passed *B. typhosus* uninterruptedly in the stools for seventy-six days after arrival at the E.F.C. Depot, the last occasion being on January 14, 1921. Since then this organism has not been isolated, but on one occasion, January 25, 1921, *B. fæcalis alkaligenes* was isolated. A blood culture was then made, with negative result.

It is possible that this man may have been a carrier of *B. typhosus* all along. But there is no history pointing to a previous attack of enteric fever, the excretion of *B. typhosus* seems now to have stopped, and here again the serological reactions are suggestive.

CASE 4 (JEMADAR M.).

Suffered from pyrexia for twelve days with diarrhœa. The spleen was palpable on deep inspiration. There was slight jaundice. Clinically this case ..s not appear to have been typical of any specific disease.

Serum received three-and-a-half months after the beginning of the disease at the E.F.C. Depot gave the following agglutinations.

B. fæc. al. (own organism)	*B. typhosus*	*B. para* "A"	*B. para* "B"
40	80	20	*Nil*

The low degree of agglutination of *B. fæcalis alkaligenes* by the sera of the above cases is noticeable and some control agglutinations with sera of men at the E.F.C. Depot were carried out for comparison, the results being as follows:—

		Agglutination
(1)	Convalescent typhoid	*Nil*
(2)	,, ,,	20
(3)	,, Para "A"	20
(4)	,, ,, (Serologically)	40
(5)	,, Enteric group	20
(6)	,, ,, ,,	*Nil*

These reactions correspond closely with those of the cases in which *B. fæcalis alkaligenes* had been isolated from the blood. In all the above reactions the organism employed was that isolated from Jemadar M.

In the first three cases noted the question arises: was *B. fæcalis alkaligenes* really the pathogenic organism or merely a concomitant of another organism of the enteric group.

Possibly *B. fæcalis alkaligenes* already present in the intestine is stimulated to increased activity by the presence of other organisms of the enteric group, as is known to happen with *B. coli communis*, and so gains admission to the blood stream.

If *B. fæcalis alkaligenes* was really the pathogenic organism in these cases, the low agglutinating power of the sera for this organism is striking.

Lecture.

ENCEPHALITIS LETHARGICA.

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(*Concluded from p.* 77.)

(2) *Nervous Symptoms.*

These may roughly be classified as follows:—(i) Cerebral. (ii) Cerebellar. (iii) Mesencephalic, including basal ganglia, pons, and medulla. (iv) Spinal. (v) Vegetative.

There is bound to be some overlapping in such a system, as several, if not all, the segments, have some influence in each nerve action.

(1) *Cerebral.*—Symptoms pointing to *meningeal irritation* are common, and all the symptoms of meningitis may be simulated, except those due to persistent high intracranial pressure, e.g., choked disk. Stiffness of the neck and cutaneous hyperæsthesia are common, and Kernig's and Brudzinski's signs of meningitis are frequently positive in encephalitis lethargica.

Motor Cortex.—Jacksonian fits occur in a few cases, and may be followed by a cortical or subcortical paralysis. Epileptiform convulsions also occur.

Speech Centres.—Aphonia, monotony of speech with slowness or rapidity of word utterance are common. Stammering has been noted in a few cases. Aphasia also has been noted, but rarely.

Sensory Cortex.—Some of the pains and hyperæsthesias are probably cortical in origin, for the distribution does not follow the spinal root type; others, however, are distinctly of spinal type. Hallucinations dependent upon irritation of the spinal sensory cortex are not infrequent.

Higher Psychic Centres.—Mental irritability is frequent in the early stages. Delirium is usually present at some stage of the illness in all but the milder cases: Nocturnal delirium, a striking contrast to the frequent diurnal lethargy is very common, and is a point of great diagnostic significance. All stages of mental depression are met with. The converse—active maniacal states—are not infrequent. Mentality, on the whole, is slow in the vast majority of the cases, but it is not abolished; the degree of mentality present contrasts strikingly with the almost comatose appearance of the patient; this point was noted by Galen 2,000 years ago [24].

(2) *Cerebellar Symptoms.*—Most of the symptoms which might be regarded as indicative of a cerebellar lesion are more easily dealt with in the next section, i.e., spasticity, vertigo, and nystagmus. Cerebellar ataxy of a generalized nature has been described, in one case accompanied by vertigo and nystagmus.

(3) *Mesencephalic* (including basal ganglia, pons, and medulla).—These are the most frequent—so much so that at first it was thought that the pathological condition was confined to this area, and one of the proposed names for the condition was mesencephalitis. Indications of a lesion on this area will usually be found at some stage of the disease.

Somnolence.—The characteristic symptom of the disease is almost always present at some stage—in fact, one would hesitate in the diagnosis if some indication of its presence was not evident at some time. The centre for sleep, if such a name can be given, has been localized in the mid-brain near to the centre of the oculomotor nerve [25]. This fact explains the frequency with which somnolence and ocular palsies are met with in the disease. The mere presence of somnolence is by no means pathognomonic of the condition, however, for any similar lesion in this area will produce the same reaction, and somnolence has been noted in tubercular and syphilitic encephalitis, and also in true sleeping sickness or trypanosomiasis. The patient appears to be soundly asleep, but may respond to questions rapidly, may take a small part in a conversation, and take food without appearing to wake up. The somnolence is most marked in the day time as a general rule; at night delirium and insomnia are frequent.

Symptoms produced by Involvement of the Nuclei of the Cerebral Nerves.—For brevity only the more important of these will be noted :—

(*a*) *Optic Nerves.*—*Amaurosis* of a transient nature has been noted. Optic *neuritis* was observed by Symonds in four cases [26] ; until this description appeared, the absence of optic neuritis was regarded as of importance in differential diagnosis: it is evident, however, that optic neuritis is very infrequent. Optic atrophy has not been noted.

Photophobia is frequently present, especially in the early stages.

(*b*) *Oculomotor Group, including Trochlear and Abducens.*—Paralysis of a more or less transient character, or more frequently paresis, constitute one of the most frequent and important of symptoms. *Ptosis* is almost always present—it is usually bilateral and incomplete, and is a factor of some importance in the production of the facies of the condition.

Strabismus is frequent, but may be of very brief duration. Of greater frequency, and of equal diagnostic value, is a symmetrical diminution of the vertical movement of the globes: the movement being good above the horizontal, but markedly deficient or even absent below the horizontal plane.

Aniscoria is frequent and may or may not depend upon paralysis of the ciliary muscle. Paralysis of accommodation is commonly met with and this paralysis is of a less transient nature than most of the other palsies.

(*c*) *The Facial Nerve.*—The presence of signs of involvement of the facial nucleus is almost constant—though the duration of these symptoms, like all the symptoms of encephalitis, may be very short. Symptoms vary from paralysis, which is infrequent to slight paresis which has been present in all the cases I have seen. The resultant facies is of great value in diagnosis, but is very much easier to recognize than to describe: the face is smooth, wrinkles and folds being largely obliterated; the complexion is usually pale, the eyelids droop or are closed, and the mouth is a little open. Any facial movement is executed slowly and the impression given by the face is one of advanced fatigue. Whether or not, however, this facies is purely dependent upon involvement of the facial nucleus remains to be proved—for a somewhat similar facies, the so-called Parkinsonian mask, is met with in paralysis agitans, in which lesions of the substantia nigra have been described—the substantia nigra is only separated from the oculomotor nucleus by the red nucleus. In myasthenia gravis a somewhat similar facies is encountered.

(*d*) *The Auditory and Vestibular Nerves.*—Transient deafness has been noted occasionally. Tinnitus aurium is fairly frequent. Vertigo is common and may be the presenting symptom, as in one of the local cases: usually the vertigo is of vestibular origin, but when strabismus is present may be dependent upon diplopia. Nystagmus is not infrequent, but more frequently nystagmoid jerks are met with and only when the eye muscles are in strained positions and are probably due to imperfect balance in the oculomotor muscles. In some cases the vertigo and nystagmus are of cerebellar type.

(*e*) *Glossopharyngeal, Vagus and Hypoglossal Symptoms.*—More or less intense paresis of these nerves is usually present in cases of any severity, and leads to the difficulty in swallowing which is so common. Diminution of the sensibility of the larynx and pharynx with resultant diminution or absence of the cough reflex, due to involvement of these nuclei, is probably one of the chief factors in the production of the pneumonia, which is frequently aspiration type. The aphonia and dysarthria noted above are to some extent dependent upon a paresis of the hypoglossal nerve.

Spasticity is frequently noted in the muscles of the limbs or trunk and is probably due to involvement of the red nucleus or interruption of the cerebellar-vestibular or rubro-spinal tracts at some other point, or to a lesion of the lenticular nucleus. The spasticity is frequently accompanied by clonus and may be general or localized to a single group of muscles, thus Massari [27] noted a stiffness of the abdominal muscles, which in the absence of other signs, simulated the reflex spasticity of an acute abdominal lesion.

Tremor is frequently noted and is another symptom attributable to a lesion of the red nucleus.

Convulsive laughter without a visible causation, attributable to involvement of the corpus striatum has been observed by Netter, Sicard and Paraf [28].

(4) *Spinal Symptoms.*—A lower neurone paralysis, similar to that of poliomyelitis but not so acute, and chiefly involving the upper cervical levels has been observed. The symptoms of irritation of the anterior cornua of the spinal cord have also been noted—these are the fibrillary twitching and tremor met with in progressive muscular atrophy. Motor spinal symptoms are unusual, however.

Numbness, formication paræsthesias and hyperæsthesias of spinal type are more frequently observed—and somewhat persistent neuralgic pains of spinal distribution are not uncommon. This is not to be wondered at when Bashford states that the most characteristic lesion of the disease is noted in the upper cervical ganglia.

(5) *Symptoms due to Involvement of the Vegetative System.*—Hiccough complicating encephalitis appears to be a much more frequent symptom in the later cases. The epidemic hiccough noted of late in France and England is regarded by Netter, Sicard, and Paraf [29] as a manifestation of mild encephalitis. The hiccough is not a pure hiccough, i.e., it is not limited to the muscles of the glottis and the diaphragm, and only one side of the latter muscle may be involved (*v.* Note D).

Involuntary movements of choreiform, athetoid or myoclonic type are becoming increasingly frequent. They are present chiefly during the stage of the declared disease. They may disappear during sleep, but owing to their presence sleep may be prevented. They vary considerably in frequency, kind and amplitude of

movement and when frequent are very exhausting to the patient. Their classification under the vegetative group of symptoms can be disputed—but, as will be seen under treatment, some observers have noted an intensification of the movements on the exhibition of adrenalin and a marked dimunition with atropine [30].

Sweating is not uncommon and may be localized or generalized.

Salivation more or less marked is not uncommon and further attention will be given to this symptom under the discussion of treatment.

Myalgies.—Some of the pains are possibly dependent upon vasomotor involvement with anæmic spasm.

Tropic Sores.—Bed sores develop with great rapidity in severe cases and after pneumonia they may be regarded as the most fatal complications.

Retention and incontinence of urine and fæces are frequently observed in cases of any severity. Retention of urine is as frequent as incontinence. Of late cases have been noted where incontinence of urine has been the outstanding symptom [31],

The Reflexes.—The condition of the reflexes is as variable as the symptomatology, and equally dependent upon the anatomical site of the lesion. The cutaneous reflexes are commonly preserved—but Babinski's reflex is commonly obtained. The tendon reflexes may be lost or exaggerated and early loss is of some prognostic importance. The variability of the reflexes from day to day in an individual case is a point of some importance.

Clinical Pathology.—The *blood*—no abnormality has been noted in the cytology of the blood: this is of importance, for in the early stages of poliomyelitis and frequently in tubercular meningitis a well marked leucocytosis has been found. The absence of leucocytosis is important. With the exception of one series of cases noted under ætiology, culture of the blood has been negative. A Wassermann reaction of the blood should always be made and is negative in uncomplicated cases.

Cerebrospinal Fluid.—As a rule the cerebrospinal fluid is under some pressure but is clear. A slight excess of protein is not unusual. The glucose is found to be increased: Netter has found as much as 0·97 gramme per litre [32] and regards this increase as a factor of diagnostic importance; he attributed the increase to stimulation of Claude Bernard's centre in the floor of the fourth ventricle. The cell content is of interest—at the onset of the disease and of remissions there may be an increase of the lymphoid cells; these diminish as the disease progresses and in a stationary or recurring case a normal count may be found. Not a sufficient number of cases have been observed to estimate the constancy of this finding, however. In poliomyelitis an initial occurrence of polymorph cell has been noted [33] with disappearance and appearance of lymphoid cells later. In tubercular meningitis a progressive increase of the lymphoid cells is the rule, and the amount of protein is proportional to the number of cells.

The urea content of the cerebrospinal fluid has shown no changes in encephalitis: in one doubtful case of this series a finding of 0·95 per cent of urea lead to a diagnosis of uræmia, which was confirmed by the lesions found at the autopsy. The Wassermann reaction should be negative.

Urine, as a rule, shows little or no changes. A trace of albumin has been

noted. Signs of cystitis or pyelitis are not infrequent in long standing cases where there has been much catheterization necessary. The urea is normal or reduced. The ammonia-urea nitrogen ratio in one case was reduced to below the acidosis level. In two cases I found acetone in the urine. (*v.* Note E.)

The *Duration* of the disease is very variable. In 88 cases recovery followed within one month in 23 cases; within two months in 22 cases; within three months in 10 cases, and exceeded this time in 33 cases [34]. The duration is proportional to the severity of the case, with the exceptions of the severe and rapidly fatal cases. No statement should be hazarded as to the probable period of invalidity. Remissions are not infrequent, and a case at present in hospital, who apparently recovered in a week, but was kept under observation for a month, returned to hospital with a remission very soon after joining his unit. At present mild or abortive types appear to be occurring with greater frequency than in the earlier epidemics, but this may be due to the more frequent recognition of the disease.

Sequelæ.—An alteration in the mental condition of the patient of a retrograde nature may persist for long, or may be permanent. Persistence of the various palsies has been noted, but is infrequent. The completeness of recovery is astonishing when the degree of prostration and the extent of the symptomatology are considered.

Prognosis.—The individual prognosis must be guarded; an apparently mild case may become severe, and an apparently severe case may recover. Of serious prognostic omen are: deepening of stupor, the onset of coma, abolition of the tendon reflexes and the occurrence of complications such as bed sores, pneumonia or signs of cardiac involvement. Death is most frequently due to pneumonia, septic absorption or pyæmia from bed sores, or involvement of the respiratory or cardiac centres. Netter gives the percentage mortality as about 40 per cent; Parsons gives a figure of 50 per cent for the English cases.

Diagnosis.—Many of the points of diagnosis have already been considered, and repetition is unnecessary. The common triad of symptoms is the presence of fever, lethargy, and localized paresis of some of the cerebral nerves. The variability of the symptomatology in each individual case is of importance. The facies is almost pathognomonic.

Differential Diagnosis may be briefly recapitulated.

Cerebrospinal Meningitis.—Investigation of the cerebrospinal fluid will show the presence of polymorph cells, the presence of specific micro-organisms and a diminution of the sugar content.

Tubercular Meningitis.—The dissociation of the pulse-temperature ratio is met with early in this condition, and only in the later stages of encephalitis. The cerebrospinal fluid shows an increase of protein, which is usually marked and a progressive increase of the lymphoid cells. The sugar content is usually diminished, but in one recent case of tubercular meningitis a normal reduction of Fehling's solution overbalanced the evidence in favour of encephalitis, which was disproved at the autopsy. The tubercle bacillus may be found in the deposit in tubercular meningitis.

Poliomyelitis.—The acute development of lower neurone paralysis after a few days of fever; the presence of an early leucocytosis in the blood and polymorph leucocytes in the cerebrospinal fluid are the chief points of difference; other differences have already been noted.

Syphilis of the mid-brain must be excluded by the Wassermann test on the blood and cerebrospinal fluid in all cases.

Trypanosomiasis, or African sleeping sickness, is not encountered in temperate climates; in an endemic area puncture of the swollen lymphatic glands, and examination of the blood and cerebrospinal fluid would be necessary to exclude the presence of the trypanosome. *Polio-encephalitis superior hæmorrhagica of Wernicke*, chiefly affects alcoholics, is not accompanied by pyrexia; delirium is more marked and more active and a greater mortality is present [35].

Botulism is not accompanied by fever, and, like other varieties of food poisoning occurs in greater or smaller epidemics with a familial or house distribution.

Treatment.—Efficient nursing is of the greatest importance—special attention must be given to the skin of the back—circular pads and frequent application of methylated spirits should be employed to prevent the formation of bed sores. The kind of diet must vary with the gravity of the case; a light, easily digested diet should be given containing much uncooked milk and raw or lightly cooked eggs; fruit or fruit juices should be given and fluids should be given freely.

Constipation may require special treatment. Retention of urine is common in the early stages and must be specially watched for; on its occurrence catheterization must be performed regularly. Where there is difficulty in swallowing, food should be given by the nasal or stomach tube.

Lumbar puncture, originally performed for diagnosis, has been frequently observed to lead to marked improvement, lasting a longer or shorter time; such an improvement was noted in all the cases of any severity I have seen.

Serum Treatment.—In 1918 Netter tried injection of the serum of convalescents, a treatment which had some success in poliomyelitis; in encephalitis the results were not fortunate, and he no longer advises its employment, for it is not yet known how long the serum remains ineffective [36]. Favourable results have been reported after the use of various non-specific sera, e.g., normal horse serum, polyvalent influenza serum, and anti-tetanic serum [37].

Urotropin is advisable, for urotropin is said to be rapidly excreted via the choroid into the cerebrospinal fluid, where it appears as formalin, and a local antiseptic action might thus occur: it is difficult to believe that a sufficient concentration of formalin could result from the small doses of the drug which can be given safely; large doses rapidly produce toxic effects, the chief being a hæmorrhagic nephritis. The urine must be examined daily for blood and albumin when urotropin is given. It is advisable that small doses be given and given frequently —for though formalin is said to appear within twenty-five minutes of oral administration it very shortly disappears. Urotropin has been given intravenously and a successful result followed in one case at least.

Sialogogues.—Netter noted that in encephalitis a swelling of the parotid glands and salivation were not infrequent symptoms of the disease, and he likens encephalitis to mumps, poliomyelitis, and rabies, in which diseases the saliva is known to be infective and the ætiology at all events somewhat similar. He therefore concluded that the elimination of the virus occurred via the saliva and attempted to increase this by the exhibition of jaborandi [38]. This conception is more interesting since de Fano has demonstrated a possible virus in the salivary glands in encephalitis—intracellular inclusions similar to those found in the nerve cells. Successful results have followed the administration of the drug, and it is

advised that adrenalin should be given as a corrective for the cardiac depression occasioned by pilocarpine.

Fixation Abscess.—The same authority has advised the injection of turpentine to produce a fixation abscess; the only known action of this procedure is to cause a marked leucocytosis. Good results are reported of this treatment. Netter quotes Hippocrates, who noted that recovery from somnolent conditions frequently followed the appearance of an abscess. According to Cheinisse this treatment was first suggested by Pic. In a series of eighty-three cases of encephalitis treated in this manner by Netter the following results are recorded: In sixteen cases the abscess did not take and there were fifteen deaths, the one recovery was a benign case. In sixty-seven cases an abscess followed injection of turpentine, and of these cases five only died. That is a mortality of 7·46 per cent of the successful and 93·6 per cent of the unsuccessful cases—and a mortality of the whole series of 24·1 per cent [39].

Atropine has been found of value in the control of the involuntary movements, in five out of six cases the action of this drug was marked [40], adrenalin had the opposite effect.

Note A.—For some of the facts here recorded acknowledgment must be made to the writings of Netter, *Presse Med.*, 1920-21, and Walshe, *Med. Science*, August, 1920.

Note B.—Marinesco, *loc. cit.*, noted the presence of similar bodies, but only in cells with no obvious nucleus; he concluded that the findings resulted from mitosis. De Fano notes the presence of the nucleus in the affected cells he describes.

Note C.—*Vide* "Contagiosité de l'encéphalite lethargique," Netter, *Bull. de l'Acad. de Med.*, No. 17, 1920. Also "Un Cas de Contagion d'encéphalite lethargique," Guillain et Leehelle, *ibid.*, p. 31.

Note D.—*Vide* a case of epidemic hiccough which after two days developed signs of encephalitis. At the autopsy typical lesions of encephalitis were found involving the upper cervical cord, and especially the third and fourth segments. Clerc et Foix, *Soc. Med. des Hopitaux*, March 18, 1921.

Note E.—Cf. Paulus Aegineta, Bk. III, *Syd. Soc. Trans.*, "Their urine is like that of cattle."

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[24] *Vide* Netter, *Presse Med.*, April 7, 1921.
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[29] *Ibid.*, December 3, 1921. (*Vide* also "L'Hermite," *Presse Med.*, December 18, 1920.)
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[31] Pfeiffer, *Berl. klin. Wochenschr.*, February 7, 1921; and Herzog, *ibid.*, March 7, 1921.
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Report.

REPORT ON "SADDLE PEAK"[1] (ANDAMANS).

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On February 18, 1882, a party organized and headed by Major Protheroe, C.S.I., Officiating Chief Commissioner, Andaman and Nicobars, made the first known ascent of the high hill on the east coast of north Andaman Islands, shown in the chart under the name of "Saddle Peak."

The expedition was undertaken with the view of ascertaining the height of the peak, the nature and general character of the country surrounding it, and whether the configuration of its summit was such as would be likely to afford a suitable site for a sanatorium.

The ascent commenced at 8 o'clock a.m., on February 18, and was made from the sea beach, up the eastern slope of the north peak, the party gaining the top at 1 o'clock p.m., thus accomplishing the upward journey in five hours; a remarkably short space of time, when the steepness of the hill-side, together with the fact that a pathway had to be cut through the thick jungle undergrowth at almost every step, is taken into consideration. Also a sharp lookout had to be kept in case the wild or untamed Andamanese in these jungles made an attack on us.

The descent by means of the track already cleared was not difficult, and only

[1] Report made on March 10, 1882.

lasted three hours. The whole journey, therefore, to the summit and down again to the place of starting, was completed in about eight hours.

The matter embodied in the following short descriptive report on Saddle Peak, and its surroundings was gathered during the ascent.

SADDLE PEAK.

General Description.—This hill (for it can scarcely be called a mountain) is situated on the east coast of the North Andamans, close to the sea, and at a point which corresponds with longitude 92° 59′ 30″ and latitude 13° 11′ 35″.

Some eight miles due north of it, lies Port Cornwallis; one of the finest harbours in the world; while thirteen miles to the south, and partly separating the north from the middle Andamans, is another superbly grand land-locked harbour known as "Stewart's Sound," and called so in honour of the present Commander-in-Chief in India, Sir Donald Stewart, Bart., G.C.B., C.I.E. The distance of "Saddle Peak" from Port Blair, which lies to the south-west, may be roughly put down at 100 miles by sea, or close on eighty miles, if measured in a direct line as the crow flies. The party travelled from Port Blair by the I.G.S. "Kwangtung" leaving Port Blair on the afternoon of February 15.

As viewed from the east, the hill appears to be made up of two lofty peaks, lying north and south of each other, with a high connecting ridge between. The outline of the whole against the sky, somewhat resembles that of a saddle, hence its name "Saddle Peak."

The north peak, or that which is supposed to represent the cantle of the saddle is the higher, it is very massive and has a smooth and rounded outline towards the top, with an easy and gradual slope downwards to the north and north-east.

The south peak, or pommel of the saddle, is not quite so high as its fellow. The outline is well defined, being sharp and irregular along the top, with an abrupt and almost precipitous decline towards the south. Deep ravines intersect the side in many places, giving to this peak a jagged and rugged appearance. The two peaks just described, are, as already stated, connected and joined together, so as to form one great hill, by a high ridge, which runs due north and south between them. The ridge is about half a mile in length, and its outline completes the body or seat of the saddle.

Its eastern side or slope is very rough and uneven, and is made up of small hills or ridges rising in successive tiers from the level land below.

A thick tree jungle, with most brilliantly tinted foliage clothes the whole hill, from the white sandy beach up to the loftiest summit, and extends also far and wide over the undulating country beneath, as far as the eye can reach.

The surface of the ground for the first three quarters of a mile in a line directly inland, from the point at which the party landed on the east coast and commenced the ascent, is smooth and level and although covered by magnificent timber and some undergrowth, yet allows of easy walking. Further on, however, the land rises rapidly, and continues to do so with varying degrees of steepness, for about a mile, when it suddenly dips and is crossed by a deep and wide ravine, along the bottom of which, a brook of deliciously cool water winds its zigzag way, dashing over and between the many large stone boulders which obstruct and line its course to the sea.

From this ravine the hill-side may be really said to take its commencement

the ascent becoming more and more decided and walking more difficult, owing not only to the sharpness of the rise, which is almost perpendicular in some places, but also in a great measure to the matting together of the numerous creepers, and young trees which compose the undergrowth.

On nearing the top of the north peak, or that which was ascended on February 18, the jungle lessens in denseness, and the surface of the ground gradually changes, and flattens out on the summit into what might be called a plateau. This level ground, or plateau, forms really the top of the hill, and is, so far as could be ascertained, more or less extensive, and well adapted for building upon, if cleared of its covering of scrub jungle, and slightly levelled where rough and uneven.

Geology.—The geological formation of the hill appears to be grey sandstone with a soil composed of yellowish earth, and a black mould rich in decaying vegetable matter.

Vegetation—Very fine forest trees of many varieties, grow over the hill-side and the rich land at its base. Of these the most valuable are the Padouk, Thitman and Peymah. Rattans, Bamboos and many species of ferns and club mosses abound towards and on the summit. A fact worthy of record and which may be of use to any one making an ascent of Saddle Peak at some future time is, that the bamboos growing near the top of the hill, are filled between their joints with a clear watery fluid, and thus, by cutting one across, a cool and most refreshing drink can be had. The bamboos proved good friends indeed to the party on February 18, as on reaching the top peak we were all very thirsty and our water bottles almost empty.

Population.—A few semi-savage Andamanese inhabit the low-lying land along the sea coast at the foot of Saddle Peak. They are very similar in appearance to their brethren in and around Port Blair but are somewhat taller and more strongly built. They live, however, after the same fashion, i.e., by fishing and occasionally hunting and killing the wild pig. They are wonderful experts with the bow and arrow and kill not only pigs but fish in this way. Their bows are very strong and will send an arrow eighty to a hundred yards. Our party all carried guns or revolvers and the six Sikhs police rifles. A party of tame Andamanese from Port Blair were sent ashore the day before our landing with presents of pigs, chickens, beads, etc., so as to make friends with their wild brethren. It would have been very dangerous to go into the thick jungle without first making friends with these little people, who are very fierce until their timidity is overcome by kindness and presents, etc.

Water.—With the exception of a few small streams at the foot of the hill, and the brook in the deep ravine before alluded to, no water was found, but judging from the configuration of the ground at the top of the hill, there can be little doubt, but that a supply of water could easily be obtained by sinking deep wells at likely spots immediately under its brow. A tank built on the high ridge already described, and constructed so as to catch the water from the two peaks on either side during the rainy season would be another means of obtaining and so storing a good supply of fresh water.

During the ascent, the air cooled perceptibly as the summit was approached, and on the top felt decidedly cold, dry and exhilarating. At the highest point reached water was found to boil at a temperature of 208·1° and 208·4°, conse-

quently (according to Thuiller's tables) showing the elevation above the sea to be from 2,300 to 2,400 feet.

Temperature and Climate.—On February 18, the temperature taken on the hill-top at 2 o'clock p.m., and at the same hour (in the shade) on board the I.G.S. "Kwangtung" (lying about a quarter of a mile off the shore) registered respectively 74° and 87°, thus making a difference in favour of the hill-top of 13°; or pointing to the air on the summit, as being by so many degrees colder than that at sea level.

Taking the temperature as above stated (74°) into consideration with the fact that at all seasons of the year, the hill has the great advantage of being exposed to a refreshing sea breeze, the climate at the top of Saddle Peak should be cool, pleasant and invigorating throughout the year and more particularly so during the north-east monsoon, or the period comprised between October 1 and May 31.

Conclusion.—In conclusion it may be stated, that so far as regards the geographical situation, geological formation, configuration of the summit (as to building and drainage) temperature of the air and the water supply, Saddle Peak is well adapted in all these respects for the site of a sanatorium, and looking at it and the surrounding country from an agricultural point of view, it is certain that if cleared and properly cultivated, abundance of fine grass and immense crops of sugar cane and rice could be raised from its rich and fertile soil, coffee, tea and tobacco, no doubt, would flourish, and give ample returns if planted along the higher slopes of the hill itself.

The trees felled in the clearing of the jungle might pay in a measure, for the labour expended thereon, especially as many of them are said to be of an exceedingly valuable description of timber.

Good and well sheltered anchorage can at all seasons of the year be found in Port Cornwallis Harbour, and a road up the hill, starting from the southern shore of this harbour, would be easy of construction, as the land from this point has (as already stated) a very regular and gradual rise towards the peak.

Current Literature.

Epidemic Hiccough. J. Lhermite in *La Presse Médicale*, December 18, 1920, p. 916.—According to Lhermite the first appearance of the present type of epidemic hiccough was observed in Vienna during the winter of 1919-20 by Economo who wrote about it as follows: "Some weeks before the appearance of the January (1920) epidemic of encephalitis in Italy, there was in Vienna and its neighbourhood a small epidemic of hiccoughs. Very numerous were the people who were suddenly attacked without preliminary symptoms, the attacks lasting for hours, or even days, and no treatment could master them. I saw a case which lasted a month, the patient having only a few hours' respite each day. This illness had no serious consequences." About a month after the cessation of this outbreak there occurred in Vienna the first cases of a myoclonic form of encephalitis in which clonic convulsions and hiccoughs were followed by a state of choreic agitation resembling the most severe forms of acute chorea. Economo wrote: "It is not possible to say that the epidemic of hiccough was already a precursor

of this encephalitis with a particular localization in the region of the phrenic vagus, but future observations on the possible relationship will be worth noting."

In January, 1920, a small outbreak of epidemic hiccough (with one fatal case) was observed by Dufour in Paris, and about the same time R. Benard reported on a similar epidemic near Versailles, and Staehelin on an epidemic at Gerlafingen in Switzerland. In this last instance the sequence of events was first an epidemic of transitory ocular paralyses, then an epidemic of hiccough, finally many cases of epidemic encephalitis.

Apparently epidemic hiccough did not again attract attention until September, when P. Gautier, of Geneva, reported on five patients who suffered from intense spasmodic hiccough almost uninterruptedly for from two to four days, the condition being quite unrelieved by treatment. The cases occurred during an outbreak of encephalitis, but he noted that they were not accompanied nor followed by myoclonic phenomena.

Early in December Logre, Heuyor, Sicard and Paraf reported many cases of epidemic hiccough in Paris, some in persons in good health, others preceded by nasopharyngeal catarrh. In a discussion following the papers by these observers Netter recorded cases which had come under his own observation, and stated that the ætiological relationship of the disease with encephalitis lethargica did not seem to him to be doubtful.

Summing up the opinions of observers who had discussed the nature of the disease, Lhermite noted: (1) Economo recognized the interest of the coincident appearance of pandemic hiccough and the myoclonic form of encephalitis lethargica, but refused to affirm the identity of the nature of the two diseases. (2) Logre and Heuyor expressed the view, with reserve, that epidemic hiccough represents a phrenic localization of the influenza virus. (3) Dufour, Renard, Staehelin, Sicard and Paraf, Netter and others, definitely considered that epidemic hiccough is a masked form of encephalitis lethargica.

Lhermite considers that the epidemiological argument to the effect that epidemics of hiccough frequently precede or are coincident with the graver manifestations of a form of myoclonic encephalitis favours the view of identity, but he lays more stress on the clinical argument that hiccough is by no means an exceptional symptom of encephalitis lethargica. He quotes Flexner, Reilly, Hunt, Gerstman and Dimetz in support of this view. Chiefly on clinical grounds he arrives at the conclusion that "an observer would show himself very severe if he refused to agree as to the original identity of epidemic hiccough and myoclonic encephalitis."

The following are notes from other articles on this subject:—

M. Rivet reported thirty cases of epidemic hiccough observed during a week, all except one being in men. Two of the cases were in persons working side by side in the same office and two were in a father and son.—*La Presse Médicale*, December 15.

MM. Rivet and Lipschutz reported the case of a man who at the beginning of December suffered from epidemic hiccough which lasted three days and was followed some days later by marked symptoms of lethargic encephalitis which were still present in the middle of January. M. Sicard reported that he had also observed two cases in which hiccough was followed by a typical encephalitis and stated his opinion that epidemic hiccough is a monosymptomatic form of myoclonic encephalitis.—*La Presse Médicale*, January 19, 1921, p. 57.

C. Massari recorded six examples of epidemic hiccough which were wrongly attributed to abdominal affections requiring surgical treatment. One of the patients succumbed after laparotomy had been performed and the autopsy showed a typical picture of the lesions of acute encephalitis.—*Wiener klinische Woch.* No. 33, 1920, p. 214.

From the Swiss Eidgenossischen Gesundheitsamtes a circular has been issued to the Sanitary Authorities of the various cantons noting the occurrence of

epidemic hiccough in the Bernese Jura and requesting all medical officers to notify cases. The circular contains the following sentence: "Epidemic hiccough belongs to the myoclonic syptom-complex of encephalitis lethargica; it is observed either in connexion with other symptoms of this affection, or by itself as a masked form of the disease. This fact has already been established in Vienna, Berlin and Paris and during last year also in Geneva."—*Bulletin des Eidgen. Gesundheitsamtes*, January 22, 1921, p. 29.

In the *Lancet*, January 22, p. 171, Dr. Sarkies records particulars of a severe case in England. It began on the fourth night after the onset of a chill, accompanied by rise of temperature and explosive cough. The hiccough lasted six days.

Dr. Jenkins in the same journal records finding, in the nasal secretion from a case, almost a pure culture of a bacillus "indistinguishable microscopically from the influenza bacillus."

Railliet reports about 130 cases at Rheims. The epidemic began on November 10, and reached its height at the beginning of December. The average duration of each case was two to four days. Children were apparently immune.—*Bull. Soc. Méd. des Hôp. de Paris*, December 30, 1920.

Netter is reported to have said that a recent English publication recorded only about 309 cases of encephalitis lethargica as having been notified in France but that in his opinion the true number would approach 10,000.—Seance du December 17, 1920, de la Société Médicale des Hôpitaux. Reported in the *Paris Medical*, January 8, 1921.

The "Dysentery Amœba" in England.—In a "Report on the occurrence of Intestinal Protozoa in the inhabitants of Britain" (M.R.C. Special Report series, No. 59, 1921), Mr. Clifford Dobell records in great detail the arrangements and the results by which we are now aware that none of the common intestinal protozoa of man has as yet disappeared entirely from England. A discovery of the same kind was made some years ago in regard to the protozoon which causes malaria. Of the intestinal protozoa which may occur indigenously in England only the *Entamœba histolytica* (the so-called dysentery amœba) is facultatively pathogenic, and the new knowledge which emerges from the present series of researches corrects the erroneous belief that infection with this parasite invariably, or even usually, results in dysentery. We must now believe that although about 7 per cent of the inhabitants of Britain harbour *E. histolytica* (as a consequence, in Mr. Dobell's words, "of having previously swallowed particles of the fairly fresh and damp excrement of other persons"), almost all of them tolerate it without any apparent ill-effect. Mr. Dobell therefore concludes that the prevalence of the parasite in England "need not be regarded with alarm," but on the evidence presented in the report he still holds the view that in certain rare circumstances, or in certain abnormally susceptible individuals, the parasite is harmful and even dangerous. "Consequently it will be necessary in future for British physicians to remember *E. histolytica* when they are called upon to treat cases of dysentery, chronic diarrhœa, ulcerative colitis, or other intestinal ailments which may possibly be due to this parasite; and to remember it also when they encounter patients displaying symptoms of hepatitis, hepatic abscess, and similar disorders in the causation of which *E. histolytica* plays a part." . . . Fortunately modern methods of diagnosis have made it possible to recognize the amœbic diseases with certainty.

Experimental Transmission of Yellow Fever. By R. P. Groves, M.D., *Journal of American Medical Association*, vol. lxxvi, No. 6, February 5, 1921.—Intra-peritoneal injections of the blood of yellow fever cases were made in guinea-pigs, the blood being taken on the second or third day of the disease and from one to three cubic centimetres being used. In two out of fourteen cases was success obtained.

In later experiments using larger amounts of blood and employing young starved guinea-pigs three out of seven experiments were positive.

The investigation followed along three lines :—

(1) Transmission of yellow fever to guinea-pigs.

(2) Passage of yellow fever in experimental animals.

(3) Production of yellow fever in guinea-pigs by cultures of the spirochæte.

The symptoms in man and experimental animals are comparable: these are fever, icterus, hæmorrhages and urinary disturbances, and at post mortem fatty degeneration of the liver and kidneys.

Pig No. 14 developed fever after an incubation period of three days, followed two days later by jaundice and death on the following day. Blood from this animal produced the same disease in other guinea-pigs and a pure culture of the spirochæte was also obtained from the blood.

The heart blood showed numerous leptospiræ on dark-ground examination and they were also found in films made from liver and spleen cultures.

A guinea-pig inoculated with twenty-five cubic centimetres of a pure culture of the leptospira developed fever on the third day, jaundice on the fourth day, and died on the fifth day when the blood was seen to be crowded with spirochætes; pure cultures were again obtained from this heart blood.

A twenty days old culture produced intermittent fever in a guinea-pig with jaundice followed by death; but a five weeks old culture produced the usual type of fever, jaundice and death on the fifth day.

The author states that pure cultures of the yellow fever spirochæte *Leptospira icteroides* have been obtained from the blood of patients as well as from experimental animals.

Review.

Gout. By Ll. Jones Llewellyn, M.B.Lond. London: W. Heinemann, 1920. Pp. xviii + 469. Price 30s. net.

In his preface the author draws attention to the rôle of bio-chemistry in the study of gout, and several of the earlier chapters are devoted to a consideration of the work of various investigators on these lines.

Then follows a discussion of the various theories of gout and the case for an infective agency as the exciting cause in the presence of some innate tissue peculiarity is effectively put forward. The author insists on heredity as the preponderating, if not the sole, predisposing factor.

The clinical account that follows is full and conveniently subdivided, as is the question of diagnosis. In regard to the latter the dictum, "Uratic deposition is the solitary unequivocal clinical criterion of gout, and in the absence of tophi the diagnosis of gouty arthritis is presumptive" emphasizes a valuable diagnostic truth.

Given the prominence of the infective theory, it is natural that eradication of local foci such as septic teeth should come first in the section of treatment. As regards food, the author claims no stereotyped regime, and points out that gastro-intestinal asepsis is the aim of all dietetic measures. As regards alcohol, he advises gouty parents to bring up their children as total abstainers, and urges abstinence or abstemiousness on the victims of the malady.

The author holds to the recognized drugs, particularly colchicum, but urges the use of atophan as a valuable aid in the prevention of the formation of uratic deposits.

Spa treatment is considered in the final chapter, the general principles and indications for selection being summarized.

Notices.

EDITORIAL NOTICES.

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Original Communications.

PRELIMINARY REPORT ON THE USE OF A SUBSTITUTE FOR THE WASSERMANN REACTON IN THE SERUM DIAGNOSIS OF SYPHILIS.

By Brevet Lieutenant-Colonel H. MARRIAN PERRY,

AND

Major E. C. LAMBKIN.

Royal Army Medical Corps.

Several attempts have of late been made to devise a more simple laboratory test for the diagnosis of syphilis than that afforded by the Wassermann reaction. That this should be so is not surprising in view of the many difficulties attendant on this test which call for no little degree of skill and experience on the part of the operator.

It is obvious that the complexity of the Wassermann test depends on the employment of the hæmolytic system; if this can be abolished, without invalidating the accuracy and delicacy of the reaction, a very considerable advance will have been made towards the simplification of the laboratory diagnosis of syphilis.

The substitution, by Sachs and Georgi [1], of a flocculation test, based on the fact that a syphilitic serum will cause flocculation of the suspended lipoids when added to a saline suspension of alcoholic heart extract and cholesterin, has recently attracted much attention. These investigators used a fixed quantity of serum which they added to a saline suspension of alcoholic heart extract and cholesterin, incubated the mixture for eighteen to twenty hours at 37° C., and then observed whether or not flocculation of the suspension had occurred—definite flocculation indicating a positive result, its absence a negative reaction.

The figures of a very considerable number of tests of this reaction are

now available for analysis [2, 3, 4], and the general conclusion is that results comparable with those yielded by the Wassermann reaction are obtained in eighty to ninety per cent of cases.

In spite of this high percentage of positive findings there are certain fallacies connected with the test which have prevented its general adoption in this country. Firstly, owing to the length of incubation necessary to ensure the delicacy of the reaction a number of false positives were given which could be attributed to bacterial contamination of the serum, causing a pseudo-flocculation of the suspension. Secondly, the fact that only one dilution of the serum was employed resulted in positive reactions being missed which would have become apparent in more concentrated dilution. Thirdly, the degree of opalescence of the suspension of heart extract was such as to cause difficulties in deciding whether or not flocculation had occurred in certain cases yielding weak reactions.

In view of the above limitations it is evident that the Sachs-Georgi test, in its original form, could not be employed to supplant the Wassermann reaction unless certain modifications, controlling these objections, could be evolved.

Dreyer and Ward [5], in a recent paper, describe a technique, based on the same principle of flocculation of a saline suspension of alcoholic heart extract, which, they state, has overcome the above disadvantages. They claim that this technique, in addition to possessing as great, or even greater, delicacy than the Wassermann test, is capable of standardization, and, therefore, that repeated quantitative tests of the strength of the reaction can be made in any particular case.

We have thought it desirable to apply this test, following the exact details of the technique described by the authors, to a series of cases at the Military Hospital, Rochester Row, at the same time controlling the result obtained in each test by means of the Wassermann reaction. The patients subjected to this investigation were not chosen in any manner, but were such as came to the laboratory for examination in accordance with the usual routine; they included treated, untreated, and suspected cases of syphilis, in the primary, secondary, and tertiary stages of infection.

Before discussing the results which have been obtained it may be of interest to recapitulate, as briefly as possible, the details of the technique which is very fully described in the paper to which reference has been made.

Reagents Required.

(1) Alcoholic extract of acetone-insoluble alcohol-soluble constituents of heart muscle.

(2) Alcoholic solution of cholesterin.

(3) Sterile normal saline solution.

(4) Saline suspensions made from 1, 2, and 3.

(5) Serum, or cerebro-spinal fluid, to be tested.

The alcoholic extract of heart muscle is prepared by cutting up finely 100 grammes calf's heart muscle, which has been freed from fat, this is placed in a glass stoppered bottle and 125 cubic centimetres ordinary alcohol (94-96 per cent) is added; the mixture is allowed to stand at room temperature for five days, and is shaken thoroughly each day. At the end of this period the alcohol is filtered off. The residue is placed on filter paper and dried in the incubator at 37° C. for twenty-four hours. It is then transferred to a glass stoppered bottle, 200 cubic centimetres of pure acetone is added, and the mixture is allowed to stand in the incubator at 20° C. for seven days. The acetone is now filtered off, and to the residue is added 100 cubic centimetres of fresh acetone which is allowed to act for a day at 20° C. The acetone is again filtered off; the residue is dried on filter paper in the incubator at 20° C. for two hours, and 200 cubic centimetres of ordinary alcohol (94-96 per cent) is added. This final mixture is allowed to remain in the incubator at 20° C. for ten days, and is then filtered through filter paper. The resulting filtrate is the alcoholic extract of the acetone-insoluble alcohol-soluble constituents of the heart muscle. It is a clear, pale yellow solution which, it is claimed, will remain stable and unchanged for a long period if stored in a well-fitting glass stoppered bottle and kept in the dark at room temperature.

The alcoholic solution of cholesterin is prepared by dissolving 1 gramme of pure cholesterin in 100 cubic centimetres of absolute alcohol.

The solution is stored in the same manner as the alcoholic heart extract.

The normal saline solution is prepared by dissolving 9 grammes of pure sodium chloride in a litre of freshly distilled water, and sterilizing at once in the autoclave.

The greatest care should be taken to ensure absolute cleanliness of the glass ware used in the preparation of the saline; if this precaution is neglected the resulting solution will contain minute particles of wool, fibre, etc., which will later give rise to considerable annoyance and trouble.

Two saline suspensions of heart extract and cholesterin of different concentrations are required in carrying out the test; they should be made up just before they are required, and are prepared in the following manner:—

Five cubic centimetres of alcoholic heart extract and 0·25 cubic centimetres of cholesterin solution are first mixed in a clean, dry test tube. Of this mixture, one cubic centimetre is placed in each of two clean, dry, 100-cubic-centimetres graduated measures. The fluid is added in such a manner as to prevent splashing on to the side of the cylinders by placing the point of the measuring pipette on the bottom of the cylinder before allowing the fluid to run out.

The sterile normal saline solution is now allowed to drop from a constant height at a constant rate directly on to the surface of the fluid in the cylinders, care being taken that the drops in falling do not touch the sides

of the cylinder. The saline solution should drop from a height of thirty-six centimetres on to the surface of the fluid, and the rate of delivery should be thirty-four cubic centimetres in four minutes thirty seconds.

The more concentrated suspension is known as suspension a, the weaker as suspension β.

To prepare suspension a 10·7 cubic centimetres are added in the above manner to one measuring cylinder.

To prepare suspension β thirty-four cubic centimetres of saline are added in a similar manner to the other cylinder.

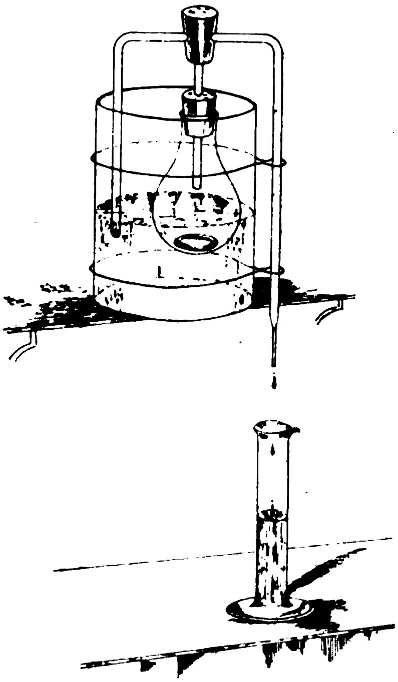

Fig. 1.

In order automatically to regulate the addition of the saline at the desired rate an apparatus has been devised by a co-worker of the authors [5], which is figured above (fig. 1). This apparatus can be so adjusted that a constant rate of drop can be maintained. It is arranged to drop at a rate which will deliver thirty-four cubic centimetres in four minutes thirty seconds. The easiest way, therefore, to measure the amounts of saline, is to allow it to drop for one minute twenty-five seconds in the case of suspension a, and for four minutes thirty seconds in the case of suspension β.

The principle of the apparatus figured above is a floating syphon. The syphon is carried on a glass bulb which is weighted with mercury and floats in a cylindrical vessel containing the saline solution. The height of the syphon above the glass float is regulated by means of a glass rod

which can be raised or lowered by sliding it through the cork closing the float, and thus varying the head of fluid and therefore the rate of dropping.

In order to produce from time to time saline suspensions of comparable sensitivity to flocculation, it is essential that they should be prepared exactly in accordance with the above directions. When the same heart extract and cholesterin are employed, suspensions can be made in this manner which do not vary in their opacity or sensitivity, and which can be used as a quantitative test to determine any variation in the flocculating power of a given serum.

Owing, however, to the variation which may occur in sensitivity of heart extracts prepared at different times, it is necessary to compare the relative sensitiveness of suspensions made with different extracts, and also with the original extract employed by the authors in the interpretation of their results. By means of this comparison each new extract can be given a "suspension factor," indicating its sensitivity which is employed, as will later be seen, in expressing the results of the test.

The serum to be tested should be obtained with strict aseptic precautions by withdrawing five cubic centimetres of blood by vein puncture into a sterile six-cubic-centimetre centrifuge tube. Care should be taken that the needle, if kept in alcohol, is thoroughly dried by passing through a flame before use, as the presence of any traces of alcohol may invalidate the result.

Before testing, the serum should be heated in a water bath to 53° to 54° C. for ninety minutes. This does not, as is the case in the Wassermann test, produce any weakening of the reaction of a positive serum, but appears to diminish very definitely the inhibition zone phenomenon which is a marked feature in this test.

A marked degree of hæmolysis, or even a moderate degree of opalescence of a serum, does not in any way interfere with the reaction, but the test must be repeated when there is obvious bacterial contamination of a serum giving a positive reaction. It has been found that a positive serum when infected may give a misleading strong reaction, and, what is even more important, a negative serum may, under the same circumstances, yield a positive reaction.

Cerebro-spinal fluids are collected with aseptic precautions, and are, in the same way as serums, heated to 53° to 54° C. for ninety minutes. The same fallacy regarding positive results due to bacterial contamination apply, but as evidence of bacterial growth is very clearly evident in a cerebro-spinal fluid, it should not arise.

Apparatus required for the Test.

(1) Standard agglutination outfit.

(2) Water bath working at 37° C.

The agglutination outfit, used in Dreyer's standard method of agglutination technique, is used in the test. This consists of an agglutination stand

provided with three rows of five holes each to hold the standard agglutination tubes, and four larger holes for the dilution tubes.

The dilutions of serum employed are effected by the drop method by means of the standard dropping pipette supplied with the outfit. The sterile normal saline solution is made up in the manner already detailed.

The necessity for absolute cleanliness of all the tubes used in the test cannot be too strongly emphasized, and if the surface of the tubes is scratched in such a degree that the reading of the result of the test is rendered difficult, they should be discarded. The best method of preparing the tubes for use is a preliminary treatment with hydrochloric acid; the acid is got rid of by thorough rinsing with distilled water, this is drained off, and the tubes are sterilized in the oven.

Method of setting up the Test.

In the case of the first test of an unknown serum, or repeating the test of a serum which has previously given a weak reaction, it is only necessary to employ five dilutions of serum.

Five agglutination tubes are placed in one row of the stand, and with the dropping pipette held vertically the requisite number of drops of saline are placed in each tube. The appropriate number of drops of serum are, in the same manner, added to the tubes, and finally the saline suspension of heart extract and cholesterin.

The number of drops of saline, serum and saline suspension of heart extract added to each tube, together with the resulting dilutions, are given in the diagram below (fig. 2).

	1	2	3	4	5
Drops of Saline	0	0	5	8	9
" " Serum 1/1	20	10	5	2	1
" " α Suspension	6	β suspension 15	15	15	15
Resulting dilution —	1 in 1·25	1 in 2·5	1 in 5·2	1 in 13·1	1 in 26·4

Fig. 2.

When the various reagents have been added, each tube is thoroughly shaken, beginning with the tube containing the highest dilution, i.e., the tube on the right-hand side of the row.

Two control tubes are always put up, one containing 20 drops of saline and 6 drops of *a* suspension, the other 10 drops of saline and 15 drops of *β* suspension.

If, as is unusual, the limits of flocculating power of a positive serum are not reached in this first series of dilutions, the test must be repeated, using,

instead of pure serum, a 1 in 20 dilution of serum. This is made up by mixing nineteen drops of saline and one drop of serum in a small dilution tube. The saline, diluted serum and saline suspension of heart extract are then added to a series of four agglutination tubes in the proportions detailed in the appended diagram (fig. 3).

	1	2	3	4
Drops of Saline.	0	5	8	9
" " serum 1/20	10	5	2	1
" " B suspension	15	15	15	15
Resulting dilution. —	1 in 46	1 in 92	1 in 232	1 in 462

Fig 3.

In calculating the resulting dilutions in the various tubes, the variation in size of drop in the case of the saline suspension of heart extract, normal saline and serum have been taken into consideration.

If several serums have to be tested, it is convenient to add to all the agglutination tubes used in the tests the necessary number of drops of saline. The serum is then added to each series of tubes, the dropping pipette being cleaned by four or five washings with saline between the addition of each serum. Finally, the saline suspensions of heart extract are added and the tubes are shaken.

The stands are now placed in the water bath at 37°C. and are incubated for seven hours. It is important that the level of water in the bath should be kept constant from day to day and should not reach higher than the middle or lower two-thirds of the fluid in the tubes.

Reading of Results.

At the end of seven hours' incubation the tubes are read.

To observe the finer grades of flocculation of the suspension, it is essential that the reading should be made by artificial light against a black background. An electric table lamp provided with a black shade so arranged that the electric bulb does not project beyond the edge of the shade serves the purpose excellently. The observer so arranges the light that he does not look into it when reading the tubes, which are held against the edge of the shade with the meniscus of the fluid slightly above its level. A six-magnification lens should always be employed to detect the finer degrees of flocculation.

There are certain terms employed to express the different grades of

flocculation which may be observed: total minus (t –), standard (S), trace (tr) and nil (0).

Total minus describes the degree of flocculation where the flocculi are large, widely separated, and some have subsided to the bottom of the tube; in addition there is marked clarification of the fluid.

Standard indicates that degree of flocculation readily seen with the naked eye. The flocculi are discrete, of a uniform size, and are evenly distributed through the fluid.

Trace is the term applied to a very fine degree of flocculation just visible to the naked eye, but readily seen with a six-magnification lens

These various grades of flocculation are taken into consideration in arriving at the results of the test, as it is necessary to determine the dilution of the syphilitic serum at which *standard* flocculation occurs. As there is a great increase in the dilution of the serum in successive tubes, it is possible that no tube in the series will exhibit standard flocculation. The approximate dilution at which this degree of flocculation would have occurred, if a larger number of serum dilutions had been employed, can be calculated by means of an interpolation table (Table I) in which the various grades of flocculation mentioned above have been given their respective values.

TABLE I.

t –	= 2·00
S	= 1·00
tr	= 0·58
0	= 0·25

The following example illustrates the method of application of this interpolation table, presuming that no tube in the series put up shows standard flocculation :—

If, for instance, the readings for the first series of tubes (fig. 1) are as follows: tube 1 t –, tube 2 t –, tube 3 t –, tube 4 t –, and tube 5 tr., the figure of dilution at which standard flocculation would have occurred lies somewhere between the dilution in tubes 4 and 5. The dilution of serum in tube 4 is 13·1, and this figure, multiplied by the interpolation figure for t, = 26·2. In tube 5 the dilution of the serum is 26·4, and this, multiplied by the interpolation figure for tr, = 15·3. The mean of these two figures—20·7—indicates the dilution which should be recorded for standard flocculation.

Attention must be directed to the very frequent occurrence of "inhibition zones" in this reaction. They are usually most marked in the serums giving the highest titre of flocculation, and may commonly extend in the first series of tubes as far as tube 5. It is, therefore, to be noted that before pronouncing a serum negative, the first five tubes must always be examined. As has already been mentioned, the heating of the serum to 53° to 54° C. for ninety minutes before testing has the effect of markedly diminishing this inhibition zone phenomenon and enhancing the clearness of the reading. Finally, where there is any doubt as to the occurrence of

the finer degrees of flocculation, the tube under inspection must always be carefully compared with the controls.

Expression of Results.

The results of the test are expressed in standard units—flocculation units—in the same manner as in Dreyer's standard agglutination technique the final readings of the tests are recorded in agglutinin units.

To arrive at the number of standard flocculation units in a serum the authors start on the following primary basis :—

"The amount of serum which, when made up to one cubic centimetre volume with normal saline solution, causes standard flocculation on being mixed with 1·5 cubic centimetre of a particular β suspension and maintained at 37° C. for seven hours, is assumed to contain four (4) units."

To calculate, therefore, the number of flocculation units in any given syphilitic serum, it is first necessary to have the sensitivity of the heart extract employed in the test standardized, i.e., its "suspension factor" determined, and to multiply the dilution at which standard flocculation occurs by this suspension factor.

For instance, to express the result of the test in the example given above of the application of the interpolation table. It has been calculated from the readings recorded that standard flocculation occurred in a dilution of 20·7. Assuming the heart extract used to have a suspension factor of 1·5, the number of flocculation units in the serum examined would be $20{\cdot}7 \times 1{\cdot}5 = 31$. As the authors have proposed to call the test the Sigma reaction, these units would be termed Σ units (Sigma units).

Interpretation of Results.

The authors publish the results of their examination of 1,077 serums and cerebrospinal fluids by this method. These included clinical cases of syphilis, cases of suspected syphilis, patients suffering from various other diseases, and healthy normal individuals. In each case a dual examination by this method and by the Wassermann reaction was made. The results were negative by both methods in 684 cases. Of 386 cases with definite evidence of syphilis the Wassermann reaction gave a positive result in 327 cases, whilst the Sigma reaction yielded a positive result in 381—i.e., in eighteen per cent more cases than the Wassermann test. In 5 cases with no history of syphilis the Wassermann reaction was positive in 4 cases, the Sigma test was negative in all 5 cases.

From these series of cases they have formed a basis for the interpretation of the results of the test which is as follows :—

(1) *Cases with no Definite History of Syphilis.*—"Serums containing less than one unit are considered to give a negative reaction. Serums containing four or more units are considered to give a positive reaction. Serums containing from one to four units should not be definitely accepted as positive until the test is repeated and confirmed, owing to the fact that

infection of the serum may have been overlooked at the time of setting up the test."

(2) *Cases with a Definite History of Syphilis*—"Serums containing less than one unit are considered to give a negative reaction. Serums containing more than one unit are considered to give a positive reaction."

(3) *Cerebrospinal Fluids.*—"These fluids, if containing less than one unit, are considered to give a negative reaction. Cerebrospinal fluids containing more than one unit are considered to give a positive reaction."

Results and Conclusion.

In this preliminary investigation, repeated quantitative estimations of the flocculating power of the serum in any particular case of syphilis have not been attempted, but the test has been applied solely to arrive at the comparability of the results obtained with those yielded by the Wassermann reaction. As has already been mentioned the cases examined were not chosen in any manner, and included treated and untreated patients in very varied stages of the disease.

Unfortunately, opportunity has at present been lacking of comparing the sensitivity of the heart extract we have employed with that used by the authors in their series of tests. We have, however, adopted the following arbitary standard in the interpretation of the results of our examinations. Before including in our series any serum as positive, it must have, as a minimum, been capable of producing standard flocculation of the saline suspension of heart extract in a dilution of 1 in 2·5, i.e., in tube 2 of the first series.

Applying this standard to the serums examined, the details of our findings are shown in the table (Table II).

TABLE II.

Number of cases	History of cases	Wa. R.	Sigma R.
94	Treated cases of syphilis without clinical symptoms which no longer gave a positive reaction	—	—
83	Treated and untreated cases with definite clinical evidence or history of syphilis	+	+
2	Treated cases with no clinical evidence, but a definite history of syphilis	+	—
2	Normal healthy individuals with no clinical evidence or history of syphilis	+	—
48	Normal healthy individuals with no clinical evidence or history of syphilis	—	—

From the above table, it will be seen that in a series of ninety-four cases of treated syphilis in which the Wassermann reaction had become negative, the Sigma test also yielded a negative result. In a series of eighty-three cases of treated and untreated syphilis, the result was positive by both tests. The observation of Dreyer and Ward that the highest unit-content of the serum is found in untreated cases in the secondary and tertiary stages of the disease was very clearly evidenced by the Sigma test, the

untreated cases could be readily differentiated from the cases under treatment by the flocculation titre of their serums.

In two cases of treated syphilis the Wassermann reaction was positive whilst the Sigma test gave negative results; in one of these cases the Wassermann reaction was returned by two independent observers as + −, a degree of complement deviation which could not be considered as a positive result in the absence of a definite history of syphilis; in the second case a repetition of the Wassermann test the following day by an independent observer was positive.

The examination of fifty normal healthy individuals without any evidence or history of syphilis was undertaken as a control. In every case the Sigma reaction was negative; in the lowest dilution (1 in 1·25) of these serums careful examination with a six-magnification lens failed to detect the slightest trace of flocculation. In two cases of this series the result of the Wassermann reaction was positive, but in both cases on repetition of the Wassermann test the result was returned as negative; this latter result was confirmed by an independent observer.

In addition to the above cases 22 cerebrospinal fluids have been examined; 6 were positive and 14 negative by both reactions, whilst 2 were positive by the Sigma test and negative by the Wassermann reaction. These latter cases gave a definite history of syphilis and were considered as clinically typical of lesions of the central nervous system. The necessity for lowering the unit content in the case of cerebrospinal fluids was well demonstrated; the strength of the reaction in a positive fluid never approached that seen in the serum.

The readings of the tests in all cases were definite and unequivocal, and no difficulty was experienced in classifying the grades of flocculation encountered in a positive serum.

In order to avoid fallacies arising from bacterial contamination causing positive results, in cases where doubt existed as to the sterility of the serum, the presence of bacterial growth was excluded by cultural tests.

The Wassermann test was in all cases undertaken by an expert in this examination; the method employed was that used at the Rochester Row Military Hospital as detailed in the Medical Research Council, Special Report Series No. 14.

In conclusion, our thanks are due to Major C. F. White, O.B.E., for facilities in investigating the cases at the Rochester Row Military Hospital, to Dr. T. E. Osmond and Mr. N. J. Clarke for their collaboration, and to Major Gordon Cummins, M.C., for much assistance.

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PART I.—*continued.*

THE INCIDENCE AND ÆTIOLOGY OF MALARIA IN MACEDONIA.

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(*Continued from p.* 108.)

MALARIA IN 1919 IN SALONIKA AND FARTHER EAST.

A WORD may be said here of malaria amongst our troops after the armistice. The greater part of the Army was moved east, and eventually occupied Constantinople, ports on the Black Sea stations in the Caucasus as far as the Caspian Sea, and even farther east. Some of the troops, however, remained in Salonika. Here they were concentrated at the base, and they no longer occupied the pestilential Struma valley or the other bad malaria districts up country. After the armistice practically every case of malaria was sent home to England.

In Salonika itself during 1919, malaria appeared as usual. There were many relapsing cases, but also many primary ones, and later in the year malignant tertian infections were noted. Several bad cerebral cases occurred, and these originated, as did most of the malaria, chiefly in the area along the Monastir road occupied by the large ordnance depot, which we have already considered from the mosquito point of view.

Many of the primary cases were amongst the Maltese labourers who arrived in the country in 1919, and this serves to show that, quite apart from the really bad spots like the Struma valley, there was plenty of malaria in the near vicinity of Salonika itself, especially east of the town.

As regards the troops that left Salonika for other countries they continued to suffer from relapses, as was expected. In Constantinople and its immediate surroundings there was very little malaria. *A. maculipennis* was found breeding pretty freely in the "sweet waters of Europe," which run into the Golden Horn, but very little malaria resulted.

On the Asiatic side along the Bagdad railway the troops were camped on country which sloped gradually into low hills. Here *A. maculipennis*, as well as *A. superpictus*, was taken, and some cases of undoubted primary malaria occurred during the summer. Primary malaria due to *A. maculipennis* also occurred in a small unit occupying a fort on the Asiatic shore of the Bosphorus near the Black Sea. All these cases, or at any rate the majority, were due to *P. vivax*. After the Salonika experiences, Constantinople, as regards malaria and even dysentery, was a veritable health resort.

In the Caucasus at Batoum on the Black Sea there was more malaria.

The town area was flat, while hills rose in a semicircle behind it. There was plenty of vegetation here and the rainfall was remarkably high. Much swampy ground occurred in and about the town, and in May, 1919, I found fair numbers of *A. maculipennis* in the buildings. Anti-mosquito work was commenced, but later in the year I found many more anopheles and primary cases of benign tertian malaria became fairly numerous. In the hills behind *A. superpictus* was taken. Anti-mosquito work carried out here under peace conditions should give very good results.

Fig. 10.—Station on the Tiflis-Baku railway to show the mosquito tower used at night by the Staff in order to escape the ravages of mosquitoes.

Tiflis, half way between Batoum and Baku, was fairly free from malaria but this was not true of the country between it and Batoum and Baku. The railway to Baku passes through an extensive plain bounded on the north by the Caucasus mountains and on the south by those of Armenia. Through the plain run rivers, and the usual marshy tracts resulted. *A. maculipennis* was found in enormous numbers in the station buildings along this line. They seemed to prefer the latrines as a resting place. *A. sinensis* was also taken, while in the late afternoon the train, even when in motion, was boarded by hundreds of voracious *Ochlerotatus dorsalis*.[1] Malaria was very prevalent all along this route.

An interesting feature of this line was that at practically every station large towers with elevated platforms had been constructed to afford a safe

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[1] Edwards claims that *O. dorsalis* should be *O. caspius*, Pallas. *Bull. Ent. Res.*, vol. x, 1920.

retreat for the railway staff, where they could sleep at night unmolested by mosquitoes.

As an illustration of the intensity of malaria in this area the experience of 305 non-commissioned officers and men of the Warwicks, referred to by Colonel Phear,[1] may be mentioned. They were sent to occupy a place south of the Tiflis-Baku railway. A delightful site on the bank of a small stream at Varda was chosen as a camping ground and everything went well for two or three weeks, when the men began to go down with fever.

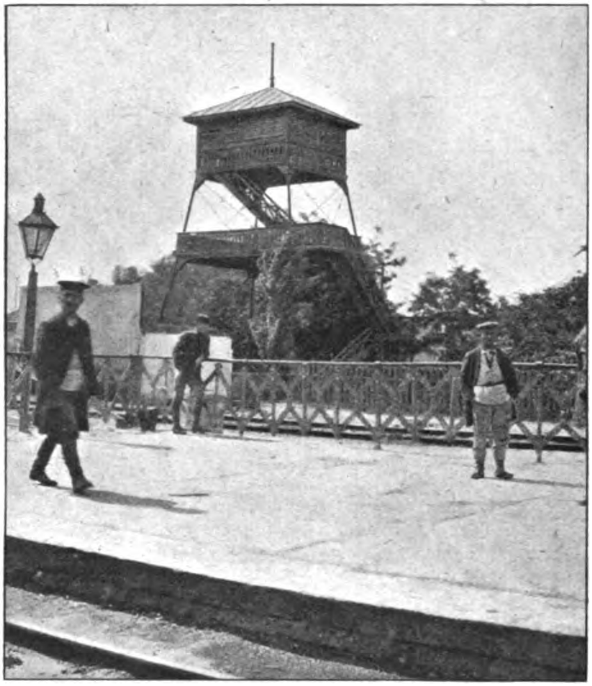

Fig. 11.—Another type of tower on the same railway. The upper platform is closed in for the use of women.

This was found to be malaria, and so many of the men were sick that the whole unit was quickly moved away to a healthy mosquito-free hill station near Tiflis. There the remainder of the men who had not yet had attacks of malaria proceeded to have them. Practically every man (303) had malaria within the next two months. Fortunately the type was benign tertian or the results might have been more serious. This instance serves to illustrate what may happen when men are exposed to infection without any protection. Needless to say, mosquito nets were not used.

[1] Phear, A. G., "Medical Experiences in Macedonia and the Caucasus." *Proceedings Roy. Soc. Medicine*, Vol. xiii, No. 7, May, 1920.

In this case we have a group of men (305) exposed to infection without prophylactic quinine and the result is that practically no one escaped infection. In Macedonia where similar exposure took place and quinine was taken, even if we admit irregularly, in no case was there such a high incidence in so short a time. It raises the question, which has been noted above, whether in Macedonia the prophylactic quinine, in spite of its reputation for uselessness, did not protect a certain, but unknown, percentage of the men.

Another rather serious outbreak of malaria, investigated by Captain Reynolds, occurred at Petrovsk, on the Caspian Sea, north of Baku. This was the headquarters of the Flying Corps and they suffered considerably from both malignant tertian and benign tertian malaria. Here again the agent of transmission was *A. maculipennis.*

It thus appears that the Caucasus is a highly malarious country and there are probably areas which are nearly, if not quite, as bad as some of the worst districts of Macedonia. Fortunately during our occupation it was not essential for us to occupy these positions, though we did so accidentally on one or two occasions, as the experience of the Warwicks shows.

The local medical authorities of Tiflis are well aware of the dangers of malaria and I was shown some remarkable posters and pamphlets illustrating the propaganda work of the Caucasian Anti-Malaria Commission. Before the disturbances of the war the question had been taken up and attempts were being made to improve the country from the malaria point of view.

Mosquitoes.

I do not intend to deal with mosquitoes in detail here. This has been done by Captain Waterston, but I wish to make some general remarks on their distribution and infectibility with malarial parasites, as this affects the incidence of the disease. Of the five, or possibly six, species of anophelines only two were of primary importance—*A. maculipennis* and *A. superpictus.*[1] In certain districts, notably the swamps near the lakes of Ardzan, Butkova, Tachinos and Besik, *A. sinensis* (*pseudopictus*)[2] had to be taken into account. This mosquito was especially common at the southern end of Lake Ardzan and fair numbers were often caught in the tents at Karasuli. For instance, between July 27 and August 4, 1917, 175 anopheles were collected for dissection from the tents here. Of these 131 were *A. maculipennis* and 44 *A. sinensis.* In no other place was this mosquito seen to the same extent. *A. bifurcatus,* first collected by Dr. Balfour at Langaza in May, 1916,

[1] The following are synonyms: *A. nursei, A. cardamitisi, A. palestinensis,* and *A. superpictus* var. *macedoniensis.* (See Waterston, *Bull. Ent. Res.*, vol. ix, May, 1918.)

[2] Edwards (*Bull. Ent. Res.*, Vol. x, January, 1920) claims that this mosquito was originally described by Pallas in 1828 and that his name, *A. hyrcanus*, must stand.

FIG 12.—The built-in spring in which *Anopheles bifurcatus* larvæ were often found.

FIGS. 13 and 14.—Type of house in Lahanah village, the lower part of which was used as a stable, in which anopheles occurred in large numbers.

occurred in some localities and its larvæ could be found, but not in large numbers, at any time through the winter, especially in the springs or fountains at the heads of the hill streams mentioned above. The new brood appeared early in the spring before the other anophelines, as they had stolen a march on them by hibernating in the larval state instead of as the adult which has to lay its eggs when the weather becomes warm enough. As a malaria carrier *A. bifurcatus* was probably of little account. It was present in such small numbers that a specimen was always regarded as a curiosity. Another anopheline (*A. algeriensis*) was still more of a rarity.

A. maculipennis and *A. superpictus* were the great malaria carriers of Macedonia. They both passed the winter as hibernating females which could be caught in any sheltered spot, caves, dug-outs or buildings, but especially in the barns and stables of the native inhabitants. The houses were frequently two-storey buildings, the ground floor being used as a store for hay and other produce, or a stable in which cattle, pigs, donkeys, goats or chickens were housed, the inhabitants themselves occupying the upper floor. Naturally a small, dark stable with its dirty, cobwebby rafters and stone walls was an ideal hibernating place for a mosquito. And when this was occupied by a buffalo the temperature was well above that of the outside air, for the animal itself was a veritable stove which warmed the air often to such an extent that the anophelines, even in the height of winter, would descend from the rafters to indulge in a feed of blood. It was shown by us that larvæ of *A. maculipennis* could be frozen in a solid block of ice for a day or more and still survive. This being the case it must often happen that larvæ of this mosquito, and probably others, survive during the winter, especially in sheltered valleys, but it would seem that the hibernating female is the chief method of survival through the winter of both *A. maculipennis* and *A. superpictus*.

When the weather becomes warm the hibernating females leave the stables and sally forth to lay their eggs. The females must have been impregnated in the autumn, for amongst thousands of hibernating anophelines collected not a single male was found. Furthermore, during the dissection of large numbers of mosquitoes during the winter it was noted that as the spring approached the eggs began to mature. For instance, mature eggs ready for laying were found in several *A. maculipennis* collected in tents at Dragos in the Struma valley on March 21, 1918, and also in a single mosquito of the same species taken at Lahanah (2,000 feet) on the same day. Well-developed ovaries with eggs nearly mature had been seen in the same mosquitoes collected at Lahanah on March 12. Mature eggs were not seen in *A. superpictus* from Lahanah till April 3. It would therefore appear that, quite apart from any difference in elevation, *A. maculipennis* is ready to lay its eggs before *A. superpictus*, and this agreed with the general experience that the former mosquito appears earlier than the latter. From April onwards the mosquitoes become more

numerous and breed rapidly in the streams and marshes. In some places during the summer they seem to suffer a check through the drying up of many streams and swamps, but by this time they have multiplied to such an extent that they occur in myriads. During the height of summer when the sun had dried up many marshes in certain places in the Struma valley or in the Vardar delta and elsewhere, a long stretch of flat country covered with brown grass would be found simply teeming with mosquitoes. To agitate the grass with a stick over an area of a few feet would be to drive out hundreds of mosquitoes. These were mostly species of *Ochlerotatus* and though they rested here in myriads there might not be any water for a very long distance. In such a spot just before and after sunset the attacks of these mosquitoes made life a misery. Even if drying up of breeding-places had taken place completely during the summer there were enough mosquitoes already to carry over till the end of the malaria season. Burning grass, which was practised on a large scale, must have destroyed inconceivable numbers of mosquitoes.

As the cooler weather approached the mosquitoes became less active and were preparing for hibernation. They collected in the barns and it was noted that the females were particularly large and healthy specimens but had undeveloped ovaries. The abdomens were bulging with fat, evidently a provision against the privations of winter.

At Orljak in the Struma valley on November 5, 1917, the buildings were found swarming with large female *A. maculipennis* evidently preparing to take up their winter quarters. In the hills the mosquitoes commence hibernation earlier than in the valley. By the middle of November the mosquitoes are in their normal winter condition. On November 25, 1918, in the middle of a cold snap large numbers of *A. superpictus* were found hibernating at Lahanah and *A. maculipennis* at Sacavca in the Struma valley.

Though I have used the term hibernation, as applied to the Macedonian anopheles, it requires some qualification. On very cold days in empty barns the mosquitoes are completely inactive. It is easy to pick them off the beams or cobwebs with the finger and drop them into a bottle. Placed in the pocket they soon revive. In warmer barns, especially in those containing animals, the mosquitoes are always more active and are caught with greater difficulty. It was found that whenever a collection was made a certain number of mosquitoes showed evidence of recent feeding. Thus of 78 *A. superpictus* taken at Lahanah on December 18, 1917, 14 had blood in the stomach; on January 21, 1918, of 98 captured 38 had recently fed; on January 23, of 49, 15 had fed; on February 9, of 61, 36 had fed. Similar figures were obtained all through the winter, which at Lahanah, 2,000 feet above sea level, was much more severe than in the Struma valley or in Salonika itself. In the valley on warm days in the winter anophelines would occasionally be caught in the open, and in buildings would attack human beings. On March 1, 1918, specimens of *A. sinensis* and *A.*

maculipennis were caught in the open at night in the act of biting at Butkova. On March 21, 1918, at Dragos, also in the valley, *A. maculipennis* was taken in the huts and tents occupied by the men and several of these had recently fed, while at Lahanah on the same date a fed *A. maculipennis* was taken in a hospital marquee. It is clear therefore that even in Macedonia, where the winter is very severe, with heavy snow falls and sharp

FIGS. 15 and 16.—Lahanah village in winter, during which *A. superpictu* was found in hibernation in the stables.

frosts, hibernation as applied to anopheline mosquitoes is a relative term. If the weather is warm enough for a day or two they will come out to feed even in the middle of winter, while in the warm barns they feed on the animals and attack human beings all through the winter, even when there is very sharp frost outside. The mosquitoes, generally speaking, retire about the middle of November. *A. superpictus* was biting very vigorously

at Guvezne during October, 1918, and amongst a batch of these mosquitoes collected at Lahanah, on November 11, 1918, many had recently fed and one contained sporozoites in the salivary gland.

It would appear therefore that in Macedonia mosquito nets should be used at least from April to November.

As regards the hibernation of anopheles a very striking feature was their almost complete absence from uninhabited villages, though exactly similar conditions existed, apart from the presence of a population both animal and human. Even in an inhabited village the mosquitoes were always more numerous in the occupied houses and barns. At Sacavca in the Struma

FIG. 17.—Bone-house at Sacavca village in the Struma valley where *A. maculipennis* hibernated in winter.

valley there was a small white building with only a door as its opening. It was used by the inhabitants as a bone house for the skeletal remains of the departed. The floor was covered to a depth of about two or three feet with human bones and skulls. Here *A. maculipennis* was found hibernating in large numbers. Though many thousands of hibernating *A. superpictus* were collected during the winter from the barns in Lahanah village not a single culex was taken. Similarly no culex was seen in the bone-house in Sacavca village where *A. maculipennis* hibernated. On the other hand, culex was more common in caves and abandoned dug-outs, where anophelines were rarely found. It would appear that the anophelines prefer a human or animal habitation during their time of hibernation, while the culex mosquitoes do not exhibit any such preference.

Distribution of *A. maculipennis* and *A. superpictus*.

As I have stated earlier in this paper, *A. maculipennis* is essentially the valley anopheline and *A. superpictus* the hill species. The former was the prevalent mosquito all along the low-lying country on the circle of lakes referred to above and all over the Vardar delta, in fact anywhere where low-lying marshy land occurred. It appeared to prefer the still water of marshes to breed in. The valley of the Struma produced myriads of these mosquitoes and every little pool or marsh appearing in the grass might be expected to yield larvæ of *A. maculipennis*. *A. superpictus*, on the other hand, was confined more or less to the hill country. It was the prevailing type all over the high land enclosed by the circle of lakes, on the Hortiak plateau and in the hill country between Lake Doiran and the Vardar river. Its breeding-places were the innumerable streams which trickled down the gullies, as described earlier in this paper. Its larvæ could be found at the highest level where such a stream occurred, 4,000 or more feet above the sea, and they extended all the way down these streams to the point where they opened out in the plain or were lost in the sandy soil before reaching it. They did not occur in actual running water but they were to be found in every tiny pool, especially those green with algæ, in the little backwaters behind the stones, in the sandy bays and in fact in any collection of water, however small or large, protected from the direct current of the stream. They were even found on the damp, sandy margin of the stream and not actually in water. Rock pools a foot or so in diameter might contain hundreds of larvæ, as also small collections of water almost completely hidden by grass. Practically all these pools contain frogs, many of them water boatmen and some of them fish, which could do little to cope with the intensive breeding of the mosquitoes.

These then are the broad lines of the distribution of the two important Macedonian mosquitoes. Exceptions, however, occurred, for there was a good deal of overlapping. *A. maculipennis* would breed high up in the hills in many places where the streams were flowing along comparatively level ground and producing still, marshy, reed-grown pools, while again in the valley a certain number of larvæ of *A. superpictus* could always be obtained. It might have been expected that they would be found in greater numbers, for they must have been constantly washed down in the streams, especially after rain. But that the main features of their distribution are as I have indicated is borne out by the following figures. Captain R. Cummins, R.A.M.C., collected 9,402 anophelines at the 60th General Hospital on the Hortiak plateau during July, August and September, 1918. Of these 9,291 were *A. superpictus* and only 111 *A. maculipennis*. Of 2,910 anophelines collected for dissection at Lahanah 2,831 were *A. superpictus* and 79 *A. maculipennis*. Of fifty anophelines taken at Dragos in the Struma valley during March, April and May, 1918, every one was *A. maculipennis*. As stated above, of 175 anophelines taken at Karasuli near Ardzan lake between July 27 and August 4, 1917,

131 were *A. maculipennis* and 44 *A. sinensis*, but the most striking instance occurred on November 25, 1918. On this day a collection made at Lahanah yielded sixty *A. superpictus* and on the same day a collection at Sacavca in the Struma valley yielded a similar number of *A. maculipennis*. November 25 had been preceded by a few days of very cold weather and the mosquitoes collected were in a hibernating condition. Many other instances of this distribution could be given but these few examples illustrate sufficiently the distribution of these two anophelines.

Mosquitoes taken at 60th General Hospital on the Hortiak Plateau, by Captain R. Cummins, R.A.M.C.

Date	*Anopheles superpictus*	*Anopheles maculipennis*	*Culex pipiens*	*Theobaldia annulata*	Other species
1918					
July 23 ..	137	0	16	—	—
,, 24 ..	286	1	0	—	*Culex hortensis*, 1 ; *Allotheobaldia longiareolata*, 1
,, 25 ..	162	3	—	—	*C. hortensis*, 2
,, 26 ..	168	2	3	—	*Taeniorhynchus richardii*, 1
,, 27 ..	268	2	—	—	—
,, 28 ..	158	1	2	—	—
,, 29 ..	114	—	—	—	*C. hortensis*, 3
,, 30 ..	38	—	—	—	— (following strong wind)
,, 31 ..	141	1	—	—	—
Aug. 1 ..	96	5	—	—	—
,, 2 ..	107	3	—	—	—
,, 3 ..	62	2	—	—	—
,, 4 ..	70	1	—	—	*T. richardii*, 1
,, 5 ..	119	1	4	—	,, 1
,, 6 ..	187	—	3	—	*Ochlerotatus dorsalis*, 1
,, 7 ..	166	2	3	—	—
,, 8 ..	200	3	2	—	—
,, 9 ..	57	1	3	—	— (following strong wind)
,, 10 ..	63	1	1	—	— ,, ,, ,,
,, 11 ..	22	1	—	—	— ,, ,, ,,
,, 12 ..	30	—	—	—	— wind and rain
,, 13 ..	21	1	—	—	— wind previous day
,, 14 ..	32	1	2	—	*Allotheobaldia longiareolata*, 1
,, 15 ..	67	3	—	—	— weather improving
,, 16 ..	56	4	2	—	—
,, 17 ..	71	2	6	—	—
,, 18 ..	98	4	2	—	—
,, 19 ..	177	2	—	—	—
,, 20 ..	115	0	1	—	—
,, 21 ..	55	1	1	—	— preceding night wind
,, 22 ..	111	0	0	—	—
,, 23 ..	166	1	—	—	—
,, 24 ..	222	—	—	—	—
,, 25 ..	139	2	1	1	—
,, 26 ..	287	0	0	1	—
,, 27 ..	340	0	1	—	—
,, 28 ..	349	0	3	—	—
,, 29 ..	133	0	0	—	—
,, 30 ..	155	1	0	—	—
,, 31 ..	136	0	0	—	—
Sept. 1 ..	60	0	0	—	—
,, 2 ..	41	0	2	—	—
,, 3 ..	79	2	1	—	—
,, 4 ..	97	1	0	—	—
,, 5 ..	25	0	0	—	—
,, 6 ..	125	0	0	—	—

MOSQUITOES TAKEN AT 60TH GENERAL HOSPITAL ON THE HORTIAK PLATEAU—*Continued.*

Date	*Anopheles superpictus*	*Anopheles maculipennis*	*Culex pipiens*	*Theobaldia annulata*	Other species
Sept. 7 ..	88	0	2	—	—
„ 8 ..	70	2	0	—	—
„ 9 ..	100	3	7	—	—
„ 10 ..	132	1	4	—	—
„ 11 ..	198	4	5	—	—
„ 12 ..	155	3	4	—	—
„ 13 ..	223	3	2	1	—
„ 14 ..	151	3	2	—	—
„ 15 ..	111	0	5	—	—
„ 16 ..	220	1	—	—	—
„ 17 ..	217	1	1	—	—
„ 18 ..	187	1	—	—	—
„ 19 ..	239	7	1	—	—
„ 20 ..	245	3	10	—	—
„ 21 ..	298	3	4	1	—
„ 22 ..	311	4	7	—	—
„ 23 ..	110	3	2	—	—
„ 24 ..	220	4	3	—	— Only two patients collecting
„ 25* ..	208	10	9	—	— „ „ „
	9,291	111			

* One *Culex mineticus* taken on 25th. It is probable that this species appeared earlier and a few specimens were overlooked and counted as *A. superpictus*.

Oct.†	2	16	2,316	2	*Allotheobaldia longiareolata*, 2

† Mosquitoes taken for five days ending October 27, 1918, at 60th General Hospital, Lembert (the hospital had just removed from the Hortiak plateau to the low area at Lembert).

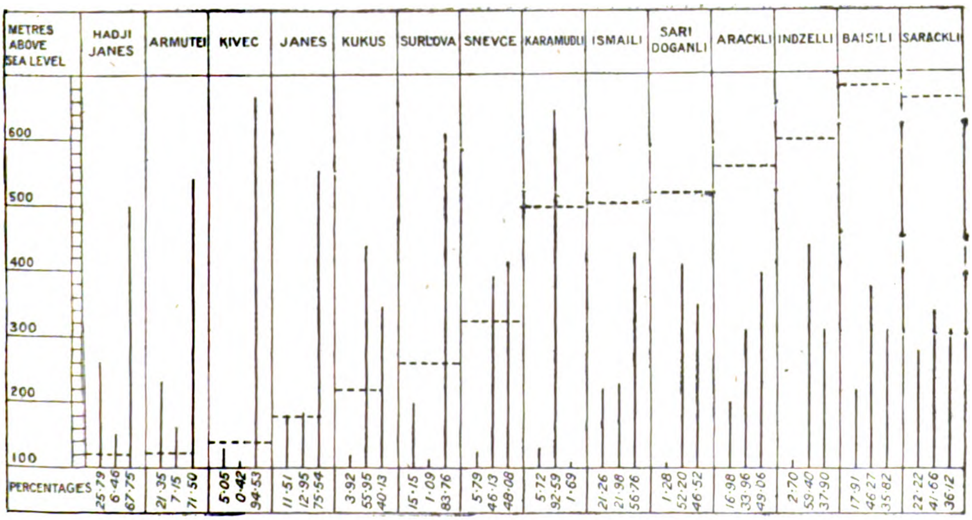

CHART II.—*Anopheles maculipennis* and *A. superpictus* and Culex collected in Janes area: influence of altitude on their relative numbers. The horizontal lines indicate altitude, the first vertical line in each column the percentage of *A. maculipennis*, the second *A. superpictus* and the third various species of Culex. The collections were made by Captain Cassidy, R.A.M.C.: *A. maculipennis*, 6,795 (10·83 per cent).; *A. superpictus*, 18,591 (30·11 per cent); Culex, 37,048 (59·06 per cent). Total mosquitoes, 62,734.

Cardamatis,[1] writing of the distribution of anopheles in Greece, makes the general statement that *A. superpictus* is the prevailing anopheline of old Greece and *A. maculipennis* that of Macedonia. What I have stated above with reference to the area occupied by our troops proves that the proportion of the two mosquitoes depends on the relative extent of hilly and low-lying valley country. From what I have seen of the mosquitoes in other parts not occupied by us, in the country west of the Vardar river, around Bralo and Itea, in Bulgaria and in the country farther east as far as Constantinople, and even in the Caucasus, the same rule for the distribution of *A. maculipennis* and *A. superpictus* appears to hold good. Hence I should be very doubtful whether Cardamatis's generalization is a correct one.

Relative Danger of *A. maculipennis* and *A. superpictus*.

The difference in distribution of these two anophelines has just been described. There still remains one other difference, and it is that *A. maculipennis* appeared first and became active before *A. superpictus*. As the former is the valley species it would be expected that it would commence breeding before *A. superpictus*, which occurred at higher elevations. However, at Lahanah it was found that the eggs of the few *A. maculipennis* taken became mature before those of *A. superpictus*, so that it seems that the former is really an earlier mosquito. In the hills, however, they occur in such small numbers compared with *A. superpictus* that from the malaria point of view they are relatively unimportant. *A. superpictus* did not become very prevalent till July, while in the valley *A. maculipennis* was numerous long before this. Again *A. maculipennis* remained active in the valley for a considerable time after *A. superpictus* had retired to its winter home. Difference in temperature will probably account for this. It thus happens that in the valleys *A. maculipennis* has a much longer season of activity than *A. superpictus* in the hills. The former is troublesome from May to November, the latter from July to October—a period which varies of course with the elevation.

As regards the incidence of malaria I have shown above that the worst places were the valleys. The sudden outbreak which occurred in June, 1916, was due to the occupation of the Struma valley. At that date *A. superpictus* had not yet become active in the hills, while *A. maculipennis* had already enjoyed two months of activity in the valley. It is thus clear that the malaria season in the valley was a longer one than in the hills. By the time that our troops had entered the valley at the end of June *A. maculipennis* had already multiplied enormously and infected itself from the native population in the villages. In July this condition of affairs was still further aggravated, while *A. superpictus* was only just beginning to be numerous and to infect itself in its turn. Though

[1] "Le Paludisme en Macédoine." *La Malariologia*. Serie 1. Anno xii. No. 4-5, 1919.

DISSECTIONS OF MOSQUITOES TAKEN FROM HOUSES AND BARNS IN LAHANA VILLAGE.

Anopheles superpictus.

Date of collection	Number	Result
1917—		
Nov. to Dec.* ..	500—600	4 positive (oöcysts only, pre-sporozoite stage)
Dec. 18—21 ..	78	*Nil*
1918—		
Jan. 21—22 ..	94	,,
Jan. 23 to Feb. 2	49	,,
Feb. 2—8 ..	85	1 positive (oöcysts only, pre-sporozoite stage)
Feb. 9—15 ..	61	*Nil*
Feb. 23 ..	30	,,
March 14 ..	89	1 positive (oöcysts only, pre-sporozoite stage)
March 21 ..	39	*Nil*
April 3—4 ..	54	1 positive (oöcysts only, pre-sporozoite stage)
April 18—21 ..	57	1 ,, ,, ,, ,, ,,
April 25—26 ..	53	*Nil*
May 11 ..	13	,,
July 22—23 ..	78	,,
Aug. 6 ..	31	1 positive (salivary glands with sporozoites, stomach *nil*)
Aug. 26 ..	35	*Nil*
Sept. 2 ..	155	7 positive (salivary glands with sporozoites, 6; stomach with oöcysts, 4)
Sept. 4 ..	92	1 positive (salivary glands with sporozoites, stomach *nil*)
Sept. 9—10 ..	265	4 positive (salivary glands with sporozoites, 2; stomach with oöcysts, 3)
Sept. 16—17 ..	270	*Nil*
Sept. 23—24 ..	218	3 positive (salivary glands with sporozoites, 2; stomach with oöcysts, 1)
Nov. 11 ..	435	1 positive (salivary glands with sporozoite, 1; stomach *nil*)
	2,281	25 positive
	550	—
	2,831	

Anopheles maculipennis.

Date of collection	Number	Result
1917—		
Dec. 18—21 ..	6	*Nil*
1918—		
Jan. 21—22 ..	3	,,
Jan. 23 to Feb. 2	2	,,
Feb. 2 ,, Feb. 8	4	,,
March 14 ..	8	,,
March 21 ..	1	,,
April 3—4 ..	3	,,
April 25—26 ..	1	,,
May 11 ..	2	,,
July 22—23 ..	24	,,
August 6 ..	7	1 positive (salivary glands with sporozoites, stomach *nil*)
Sept. 2 ..	5	*Nil*
Sept. 9 ..	4	,,
Sept. 23 ..	2	,,
Nov. 11 ..	7	,,
	79	

* The detailed notes of these results were lost.

Percentage of mosquitoes infected at Lahana on dates where infected ones were found	
Nov. to Dec., 1917 ..	0.6
1918—	
Feb. 2 to Feb. 8 ..	1·2
March 14	1·1
April 3—4	2·0
April 18—20	2·0
August 6	3·0

Percentage of mosquitoes infected at Lahana on dates where infected ones were found	
1819—	
Sept. 2	5·5
Sept. 4	1·0
Sept. 9—10	1·5
Sept. 23—24	1·3
Nov. 1	0·2

A. superpictus multiplied very rapidly during July, August and September and also infected itself during these months, it would never be able to overtake either in numbers or in degree of infection the *A. maculipennis* which had had two months' start. Hence the valley would always be a more dangerous place than the hills, even if in numbers *A. superpictus* were eventually to equal *A. maculipennis.* The number of infected mosquitoes in the valley would always be higher, owing to their longer exposure to infection. The foregoing I think is the real explanation of the much higher incidence of malaria in the valleys.

MOSQUITOES TAKEN FROM OTHER PLACES.

KABASULI (Ardzan lake).

Date	Locality	Mosquito		Result
July 27, 1917 ..	(Hospital tents)	*A. maculipennis*	25	*Nil*
,, 27 ,, ..	(80th F.A. Spankova) ..	,,	5	,,
,, 28 ,, ..	Hospital tents)	*A. maculipennis* and *A. sinensis*	34	,,
,, 29 ,, ..	,, ,, ...	*A. maculipennis*	32	,,
,, 30 ,, ..	,, ,, ...	,,	40	,,
,, 30 ,, ..	,, ,, ...	*A. sinensis*	14	,,
Aug. 4 ,, ..	,, ,, ...	*A. maculipennis*	12	,,
,, 4 ,, ..	,, ,, ...	*A. sinensis*	13	,,
	DRAGOS (Struma valley).			
March 21, 1918 ..	(Hospital tents)	*A. maculipennis*	7	*Nil*
April 22 ,, ..	,, ,, ...	,,	9	,,
,, 25 ,, ..	,, ,, ...	,,	11	,,
,, 26 ,, ..	,, ,, ...	,,	19	,,
May 11 ,, ..	,, ,, ...	,,	4	(Salivary glands *nil;* stomach, 12 oöcysts)
	LAHANA (2,000 feet).			
March 21, 1918 ..	(Hospital tents)	*A. maculipennis*	1	*Nil*
,, 21 ,, ..	,, ,, ...	*A. superpictus*	2	,,
May 7—8 ,, ..	,, ,, ...	*A. maculipennis*	4	,,
	LIKOVAN VILLAGE (near Lahanah).			
March 21, 1918 ..	(Village)	*A. superpictus*	7	*Nil*
.. .. ..	.. ..	*A maculipennis*	5	,,
	GUMUS DERE (Struma valley).			
June 4 ,, ..	(Hospital tents)	*A. maculipennis*	83	*Nil*
,, 24 ,, ..	,, ,, ...	,,	37	,,
	49TH GENERAL HOSPITAL (Hortiak Plateau).			
May 1, 1918 ..	.. ..	*A. superpictus*	20	*Nil*
Aug. 23 ,, ..	.. ..	,,	18	,,

It was very naturally suggested that some difference in the infectibility of the two mosquitoes might account for the variation, and further, as *P. falciparum* appeared later in the year than *P. vivax*, it was thought that *A. superpictus* might prove to be the special carrier of *P. falciparum*. I see the same suggestion made in reports on malaria work in Palestine.

Experiments to test this point were carried out (see below). It appeared that both *A. maculipennis* and *A. superpictus* could very readily be infected

with *P. falciparum* and that they became infected to an equal extent. With *P. vivax* both mosquitoes became infected, but *A. maculipennis* more readily than *A. superpictus*. The number of experiments was not great, but sufficient to indicate that as regards infectibility with *P. falciparum* and *P. vivax* the two Macedonian anopheles are practically the same so that this explanation of the early appearance of *P. vivax* and the late appearance of *P. falciparum* or the more intense malaria of the valleys compared with the hills cannot be sound. Furthermore, there was no greater incidence of *P. falciparum* infection from the hill country where *A. superpictus* abounded.

Incidence of Infected Mosquitoes in Nature.

A good many loose statements have been made about the intensity of mosquito infection in Macedonia. Some writers speak of areas in which practically every anopheline is a carrier of malaria. Such statements are certainly erroneous. In order to obtain some definite information a systematic dissection of anophelines from Lahanah village was undertaken. This village was chosen because it was accessible and because there was a very high degree of infection amongst the native children, as will be seen by a reference to the table in Captain McLay's paper. In all, 2,831 *A. superpictus* and seventy-nine *A. maculipennis* were dissected from this village. All the mosquitoes were collected from the native houses either in the living rooms or in the barns below, the vast majority being collected from the latter. The result showed that the highest figure was obtained at the height of the malaria season, for on September 2, 1918, of 125 *A. superpictus* dissected 7 were infected, while on August 6, of 7 *A. maculipennis* 1 was infected. Taking the figures in four-monthly periods the percentage of infected mosquitoes was as follows:—

November, 1917, to February, 1918	0·5
March, 1918, to June, 1918	0·3
July, 1918, to October, 1918	1·5
November, 1918	0·2

It was surprising, especially in the light of some of the statements that had been made, that in such a heavily infected village during the height of the malaria season only 1·5 per cent of the anophelines were infected. A great deal evidently depends on the opportunity the mosquitoes have of feeding on an infected individual, for on September 2, 1918, a batch of forty-two mosquitoes collected from one barn gave six infected, or a percentage of 14 per cent. It is highly probable that some infected child had been sleeping in this barn. It is evident therefore that large numbers of mosquitoes have to be dissected in order to arrive at a reliable percentage of infected individuals amongst them.[1]

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[1] In the paper by French authors, referred to on p. 108, the following statement is made as regards the number of infected mosquitoes. Under certain conditions, as for example around the hospitals occupied by malaria patients, nearly all the anopheles

The earliest date on which an infected anopheline was discovered in the Struma valley was May 11, when an *A. maculipennis* was taken with oöcysts in the stomach at Dragos. It would be expected that infection in the Struma valley would commence earlier than at Lahanah, which is 2,000 feet above sea level but systematic dissection of mosquitoes from the valley was not undertaken. During 1917 Karasuli, near the south end of Ardzan lake, had a notoriously bad reputation for malaria, some units losing in three months as much as 150 per cent of their strength. Of 175 anophelines taken, principally from the tents of the 28th Casualty Clearing Station, not a single infected individual was met with. It is highly probable that if the mosquitoes—many of which were gorged with blood, in some cases at least from malaria patients—had been kept for a sufficient time before dissection a very different result would have been obtained.

18.—Children of Lahanah village who were heavily infected with malaria parasites.

Infected Mosquitoes found in Winter.

An interesting result of the dissections of the Lahanah mosquitoes is that during the winter months seven *A. superpictus* were taken with oöcysts on the stomach.[1] The cysts were in the pre-sporozoite stage and

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are infected (Abrami); in a more extended survey in the hinterland (Joyeux) and in the Salonika area (Cot and Hovasse), the percentage varied between three and six (Niclot). It will be seen that the latter figures are higher than those I obtained in Lahanah, a heavily infected village, while Abrami's statement is difficult to understand when compared with the results I obtained at Karasuli (p. 186), with mosquitoes collected in the hospital where large numbers of malaria patients were constantly present, and where the incidence of malaria was particularly high.

[1] Since this article was written a paper by Sella (*Internat. Journ. Pub. Health*, November, 1920) has appeared, in which he mentions the discovery of infected anopheles in winter at Fiumicino near Rome. The percentage of anopheles found infected during the year agrees with that obtained in Macedonia.

no case of salivary gland infection was noted. The cysts had a normal appearance and the question arises whether such hibernating mosquitoes might carry cysts in a latent state till temperature conditions became more favourable for complete development. Should this prove to be the case then mosquitoes would be able to carry infection from one season to another.

To test this point mosquitoes in which partial development of *P. falciparum* had taken place were kept in the ice-chest at a very low temperature for some days and then incubated at a temperature suitable for development. In all cases this treatment resulted in a degeneration of the cysts. The conditions of experiment were probably more rigorous than those to which hibernating mosquitoes are exposed in the Lahanah barns, especially in the ones containing animals, the temperature of which is never so low as to prevent the mosquitoes from feeding.

Accordingly the experiment was repeated. After partial development had taken place the mosquitoes were kept at the ordinary laboratory temperature, which was sufficiently low at that time to prevent any commencement of development in control mosquitoes. After fourteen days the mosquitoes were returned to the incubator and it was found that not only had no degeneration of the cysts taken place but that development was completed.

It would thus appear that under certain conditions development may be completely arrested by cold, and that it will continue under more favourable conditions. It follows that the carriage of malaria through the winter by hibernating mosquitoes is a possibility.

These experiments will be considered in more detail below.

Parasite of Malaria.

In this section I propose to deal with the malaria parasites themselves as they occurred in the Macedonian malaria.

Characters.—The parasites have been submitted to very careful examination, and it can be definitely asserted that there is nothing peculiar in the morphology of any of the forms met with. They correspond in every way with those one has studied in other parts of the world. The same remark can be made of the intensity of infection. In the worst cerebral cases the parasites in the blood and various organs, as regards their numbers and characters, conform in every way with the appearances one has seen in this type of case from many other countries. All the three well-known species of parasite occurred, but *P. malariæ* of quartan malaria was exceedingly rare amongst the troops, though a reference to Captain McLay's paper will show that it was quite common amongst the native children.[1] As the native was undoubtedly the source from which our

[1] James (" Malaria at Home and Abroad," p. 102), mentions an instance of high rate of quartan infection amongst native children in India.

troops derived their infections, it is a remarkable fact that quartan infections were so rarely encountered.

On several occasions very large crescent infections were seen in anæmic individuals with large spleens. Such cases are not unknown amongst individuals who have lived in West Africa and other places. Cases with large numbers of gametocytes of *P. vivax* were also seen, and these were specially interesting as they not infrequently showed the supposed parthenogenesis forms of Schaudinn, which I had often seen before. I was perfectly familiar with these appearances, and had long maintained and taught that Schaudinn's so-called parthenogenetic forms were merely cases of double infection of cells with a schizont and a gametocyte. Dr. J. D. Thomson published a very interesting paper in which he expounded this view and refuted Schaudinn's theory of parthenogenesis.[1]

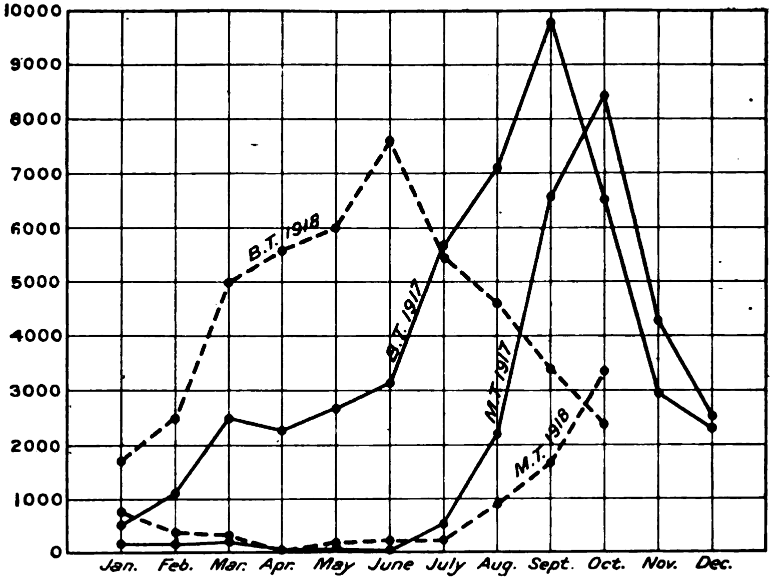

CHART III.—Calculated number of malignant and benign tertian admissions, 1917 and 1918. This curve is based on approximately 40,000 positive blood film examinations as collected from the Army laboratories by Colonel Dudgeon, C.M.G., C.B.E.

[1] Notes on Malaria. JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, vol. xxix, October, 1917.

[2] That the question of reproduction of gametocytes within the human host is still regarded as possible by some observers is illustrated by a recent paper by Pontano (*Policlinico*, Anno XXVII, Sez. med., Facs. 1, January 1, 1920). This observer states that he has seen forms of the malarial parasite in cases just relapsing after long intervals of freedom from acute symptoms, which are neither gametocytes nor double infection of a single cell with gametocyte and schizont. He cannot state whether they reproduce by parthenogenesis, as Schaudinn claimed, or whether there is fertilization within the human host, as Mary Rowley maintains, but he is convinced they are derived in the first place from gametocytes. The figures he gives of these forms still appear to me to be compatible with the double infection of a cell with schizont and gametocyte.

Double infections with two species of parasite were by no means uncommon, though they were not reported as frequently as might have been expected. The explanation for this is that during the malaria season so many blood films had to be examined that as soon as a diagnosable form was seen no further examination was made. It was for this reason

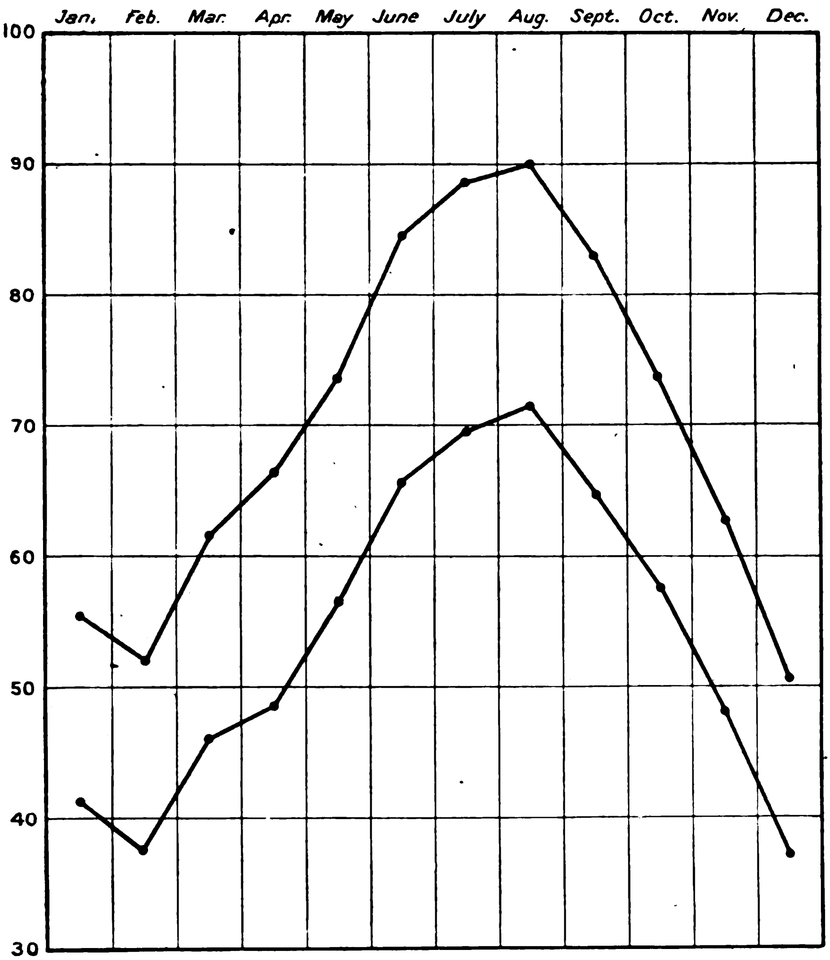

CHART IV.—Average maximum and minimum temperatures for 1917. The curves for 1916 and 1918 and for a ten years' average were almost identical. They have been constructed from the average daily maximum and minimum temperatures for each month.

that so many cases which were returned as infections of *P. falciparum* during the summer, later on during the winter or succeeding year suffered relapses in which *P. vivax* was found. The former was the predominating one at the time of examination, while the more persistent and quinine-resisting *P. vivax* was overlooked.

SEASONAL INCIDENCE OF *P. vivax* AND *P. falciparum*.

The tabulated results made by Colonel Dudgeon of the blood film examinations of the Army bacteriological laboratories revealed the fact that at the end of the year (November) about half showed *P. vivax* and half *P. falciparum*. After this period there is a fall in the proportion of the latter, and a rise in the former, till during the period March to May well over 98 per cent of the positive films show *P. vivax*.

If the positive films are taken to represent the proportion of the two types of malaria amongst all hospital malaria patients at the corresponding periods, it is possible to calculate the total number of benign tertian and malignant tertian cases at any time. This has been done for the year 1917-1918, and the result is shown in the accompanying chart, which probably roughly indicates the actual state of affairs. It shows that the benign tertian rise and fall occurs before that of the malignant tertian, while the actual number of cases of the former reaches a higher level than the latter. Another point which has to be noted is that from May onwards practically all cases of malignant malaria infection are primary cases, for reasons which will be explained below, whereas with the benign tertian a varying but considerable proportion are relapses of the previous year's infections. Some are undoubtedly superimposed infections or reinfections. An undoubted error in the curve results from the fact that during the height of the malaria season there was a tendency to examine the blood of the more severe cases in preference to others, so that the proportion of positive diagnosis of malignant tertian malaria was higher than was really correct.

(*To be continued.*)

FRAGMENTS.

By Colonel Sir ROBERT FIRTH, K.B.E., C.B.

XIX.

The last fragment was concerned with some deductive speculations on the mentality of man, more especially when he is part of a group or crowd, and the conclusion was reached that he is very open to suggestion. One of the effects of this amenability to suggestion is that irrational belief forms a large part of man's mind and is indistinguishable by the subject from rational verifiable knowledge. Non-rational judgments, being the product of suggestion, acquire or imply the quality of instinctive opinion, but opinions, on the other hand, which are the result of experience alone do not appear to possess this quality of primary certitude. They are true in the sense of being verifiable, but they are unaccompanied by that profound feeling of truth which instinctive belief possesses, and, therefore, we are not reluctant to allow inquiry into them. I do not imply or think that suggestion acts always on the side of unreason. The trouble is not irrationality and not a preference for unreason, but that suggestibility conveys a capacity for accepting reason or unreason provided it comes from the proper source, which in most cases is synonymous with its being in accord with the general opinion of the group, mass or herd. This quality seems to be a direct consequence of the social habit or instinct of gregariousness in man, which instinct makes social life possible and altruism a reality. In the preceding fragment, I referred to altruism "paying." To avoid misunderstanding, I would say that this does not mean a something in the nature of a judgment by the individual that it pays him to be unselfish. The truth is, altruism does not and cannot pay the individual in anything but feeling. Man is altruistic because he must be, not because herd suggestion recommends it, but by virtue of a natural instinct which arouses reason to work and opposes the repressing force of social habit and herd tradition.

Each of us knows that in our lives there is the element of conflict, the process beginning with childhood. The child receives from his group certain teachings such as that truthfulness is the greatest of virtues, that honesty is the best policy, that death has no terrors to the righteous, and that there is in store a future life of perfect happiness. As the child gets older, experience tells him that the untruthful and dishonest often have as good if not a better time than he, and that the religious fear death and are as determined to hold on to this imperfect life and happiness as much as the unbeliever. With advancing years the vague feeling of dissatisfaction, and the obscure or elusive sense of something being wrong which is left by these and similar conflicts, becomes even more marked. This

mental unrest is evidence of the handicap laid upon the developing mind in its attempt to assimilate the dicta of group or herd suggestion with experience. In adolescence and maturity, experience is real and not the shadowy dreams which it is usually to the child. The primitive instincts of self-preservation, nutrition and sex are now developed and become in conflict with the pronouncements of the group, and even altruism or the yearning for and to give protection has acquired instinctive force from group suggestion. The sufferings and heart-burnings entailed by this condition are common knowledge, and it is not surprising that around matters of religion and sex the conflict is most severe.

As an individual of a gregarious species, man can never be really self-sufficient. Natural selection makes him have an abiding sense of incompleteness, and this is the psychological form which expresses itself in religious feelings and the desire for completeness or incorporation with something more perfect than himself. It is much the same with the sex impulse, which is really associated with altruism, and constitutes about the only outlet by which man is allowed by the group or herd to give expression to manifestations of passionate intensity. Let us imagine the case of and the conflicts within a man enamoured with some individual forbidden to him by his group and see what are the possible developments. The conflict may end through the subsidence of either antagonist or years, other instincts or grosser passions may moderate the intensity of ungratified love. An alternative may be found in scepticism detecting the limits in value of the group suggestion and so deprive it of its conflicting force, or the problem may be shirked by rationalization and the man take the forbidden pleasure and salve his conscience by either endowing a hospital or chapel and persuading himself that in the end Mercy will be shown him. The solution by scepticism is rare, but that by indifference and rationalization or a mixture of the two is the rôle of the average man. This is the class whose stability shows them individually to possess a relative incapacity to take experience seriously, a relative insensibility to the value of feeling, and a definite preference for group tradition over other sources of conduct. Whether this class is the normal or numerically superior is questionable, but it certainly is the dominant class; whether the mental stability of which they are the type is a gain or a loss is also open to argument, but in so far as the mentality indicates a limitation of outlook, intolerance of the new in thought, and a consequent narrowing of the range over which desirable intellectual activity is possible, there is much which suggests a loss.

Outside this ease-loving, smug and mentally stable or resistive type, society is being penetrated steadily by those whose characteristic is mental instability. Whereas the stable-minded deal with an unsatisfactory experience by rejecting its significance, in many of the converse type of mind such successful extension does not occur, and the unwelcome experience persists as an irritant, capable neither of rejection nor assimila-

tion. These are the minds which are apt to develop certain mental manifestations which have usually been regarded as disease, and it is legitimate to interpret these and other manifestations of what we call mental instability as really consequences of mental conflict and due to the effects upon the mind of the failure to assimilate the experience presented to it into a harmonious unit personality. The manifestations of mental instability are not diseases of the individual in the ordinary sense, but consequences of gregariousness and man's continuous conflict between experience and group suggestion. Alcoholism is a case in point. Regarded as either, on the one hand, a vice or sin, or on the other hand, as a disease, there is much to suggest that it is but a response to a psychological necessity. In the conflict between what he has been allowed to desire and what he has been allowed to get, man has found in alcohol and other drugs an effective peacemaker. True, but a small proportion of the victims of conflict find a solace in alcohol, yet that must not blind us from appreciating how great must be the number of those whose need was as great, but who were too cowardly or perhaps too brave to find a release there.

The prominent characteristic in which the mentally unstable contrast with the average man is motive; they are weak in energy, have a vague scepticism as to the value of things in general, little will power or ambition, and readily won to new causes, new religions, new quack remedies, and as readily desert them. What the average or mentally stable gain in motive they lose in adaptability, and what the unstable gain in adaptability they lose in motive. I dare not discuss the significance of these two great mental types found in society more than to say that they both must be regarded as defective, and as evidence that our civilization has failed to provide an environment in which the average human mind can grow unwarped and to its fullest. The vast power of varied reaction possessed by man demands a power of intercommunication of unprecedented fineness if he is to attain the full advantages of his gregariousness. Since no hint of such power has appeared and that it is equally obvious that it is this defect which gives to society its present-day characteristics, we can appreciate how momentous is the question as to what our civilization does with the raw material of its minds to encourage in them the potential capacity for intercommunication which they by nature possess. There is but one answer to the question, and that is by providing its members with a herd or group tradition which is constantly in conflict with feeling and experience, society drives them either into resistiveness or into mental instability, both conditions tending to exaggerate that isolation of the individual which the intellect shows us to be unnatural and the heart indicates to be cruel. The conception that the environment of man must be modified if the body is to survive has long been recognized, but the fact that the mind is more delicate than the body has received scant notice. The price we pay for this neglect is the steady increase in numbers of the mentally unstable

among us. The remedy is not to be found under cover of the shibboleths of eugenics; we need to seek the origin of the defect, but the difficulty is how to find it.

A step towards this successful search may be to appreciate the existence among the mentally unstable of an exaggeration of the quality of sensitiveness to feeling and experience. That such sensitiveness increases with civilization is evident from the close association between civilization and mental instability, but there can be no doubt as to the value to the State of such sensitiveness were it developed in a sympathetic and suitable environment. The problem which faces us, in these days, is how to adjust the mental environment in such a way that sensitiveness may develop and confer on man the advantages which it holds for him, without being transformed into the menace of instability. This can only be effected by some radical change in man's attitude towards the mind and an extension of the rational method to the whole field of experience. We have revolutionized our conceptions of and methods of combating the diseases caused by bacteria and the unicellular organisms; in some similar way we must overhaul, remodel and improve our knowledge of treating the mind and the psychoses. The day is gone for any blind confidence in our destiny, and unclouded faith that our traditions, laws and institutions contain permanent qualities of reality. Unless we realize that we live in a world where, outside our own race, no allowances are made for infirmity, we shall prove ourselves to be, after all, but one more of Nature's failures, and have to make way for another venture of her patience.

XX.

Many years ago, Huxley was baffled by the incompatibility he found to subsist between what he called the ethical and cosmical processes as affecting man's evolution and, if the reader has followed the train of thought outlined in the last two fragments, it will be apparent that we must cease to regard man as an animal only, but explore his mind and his moral capacities in the knowledge that these are not meaningless intrusions into an orderly world but elements in the complex structure of the universe. The development of an objective attitude towards the status of man is not new and has influenced significantly the study of the human mind. The time is past when we can be satisfied with a primitive type of psychology, tainted with anthropomorphism and telling us not what man is, but merely what he thinks and feels himself to be, and producing no generalizations of value in the practical conduct of life. Neither can we rest satisfied with a psychology based either on the work of the experimental physiologist or on the abstractions of logic and metaphysics. These methods were sterile because they ignored the study of the mind and had no objective standard by which the value of mental observations could be estimated. In the absence of such a standard, any given mental

phenomenon might be either a product of the observing mind or a product of the mind observed, or even a mixture of both minds. The newer work of recent years suggests that the much needed objective standard may be found in two directions. One is by a study of pathological mental states and by an understanding of such pathological conditions to argue back to the concealed phenomena of the normal. The other is by a study of actions rather than of spoken words, or by regarding action as a more important touchstone of motive than the actor's own views. By this method there is a search for criteria whereby instinctive impulses or their derivations arising in the mind can be distinguished from rational motives.

All of us are familiar with the enormous biological and social importance of the emotions, and with the closeness of their relation to instinctive and cognitive action. The fusion of emotion with cognition has been termed a sentiment or feeling indicating a past experience which had by association with emotion left behind a cognition. Thus, a cognitive experience, whether it be a percept such as of one's watch, or a concept such as of one's home or native land has a value to us and is endowed with emotional experience and power. If I break my watch I experience the emotion of sorrow or regret; if some one belittles my native land, the emotion of anger is aroused within me. A sentiment is thus a system of past ideas or percepts, set in a group of definite emotional dispositions with which by past experience they have become associated.

The importance of association has long been known and the recognition that mental processes tend to recur, not only by virtue of their previous association with other mental processes but also by virtue of their inherent and retained energy, is the basis of the newer psychology, especially that of Freud and his school. This psychology regards the mind of the adult as the outcome of a process of development the stages of which are within limits, orderly and inevitable. The course of this development in each individual is determined by forces which are capable of precise definition, and the final product is capable of yielding to expert examination clear evidence of the particular way in which these forces have acted and interacted during the developmental process. The fundamental conception of the Freudian psychological system is that the development of the mind is accompanied and conditioned by mental conflict. The infant and young child is regarded as being impelled by instinctive impulses which at first are solely egoistic. Gradually, by contact with the world, resistance to the full indulgence of these impulses is encountered and, little by little, the external interference of social pressure becomes so formidable that a veritable condition of mental conflict is established quite early in life; egoistic impulses press for indulgence regardless of their acceptability to the environment, while influences from the social group or herd bear equally against any indulgence unwelcome to surrounding standards of discipline, taste or morality. Of the two parties in the conflict, namely, the instinctive impulse and

the repressive force, the former is regarded by Freud as being wholly the product of the sex instinct. Whether such is the case is open to argument, but there are no difficulties in accepting the conception of the significance of mental conflict, the importance of the emotional experiences of infancy and childhood in the determination of character and the causing of mental disease, or the conception of the mind as comprising conscious and unconscious fields. Neither is it difficult to accept the conception that when development proceeds normally the surplus energy of the sex instinct finds an outlet in such activities as the altruistic, the poetic or the æsthetic, all of which have a social value; when the development is interfered with the outflow of energy is liable to result in definite abnormalities or disease of the mind, or in peculiarities of character scarcely to be differentiated therefrom.

In case the reader has any doubt as to the manner in which the modern psychologist approaches this difficult question of mind, I would remind him that when he has just fallen in love or suffered a bereavement he is powerless to avoid the intrusion of the person, the scenes and the emotions related to his new experience, into his everyday life. Further, when the system of ideas with which such an emotional disposition is connected becomes inhibited or repressed, the emotion only gains force as it meets with opposition. Coincident with its repression, the system of ideas becomes a complex. The repression or inhibition is due to a conflict that represents a discordance or unpleasantness that one dares not to face. In this manner the complex is consciously or unconsciously repressed into the unconscious. It may rest there for ever unless dragged out through the agency of some mental explorations as by psycho-analysis or under hypnosis which succeed by reducing the repressing force. Or, the complex may be imperfectly repressed and the energy of the emotional part of the repressed system of ideas may escape in the form of unreasoned emotion, such as fear or anxiety, unattached to any special object or more often attached to some analogous object. When the complex is imperfectly repressed, the original scene may itself recur when inhibition is reduced, as in sleep; but even in dreams the scene may recur only in distorted form, seemingly to elude the forces of inhibition. Thus the importance of the unconscious becomes as great for psychology as that of the conscious. Into the unconscious we are ever, more or less unconsciously, banishing ideas and percepts which are discordant with our general mental life. From the unconscious emerge not only complexes or parts of complexes which have been repressed, but also new formations, such as the creations of the genius or inventor, which are then presented for judgment and elaboration to the conscious mind.

These modern developments of psychology are independent of physiology. We have no idea of the neural seat or the physiological conditions of consciousness or unconsciousness. It is doubtful whether

there is a transference of physiological activity from higher to lower neural levels, as when a conscious act, by sufficiently repeated experience, becomes an unconscious habit. There is much to suggest that under certain conditions consciousness may be present, while under others it may be absent when the same nervous areas are thrown into activity. We have no reason to believe that those parts of the brain which physiology indicates to be associated with sensation and perception are the seats of conscious activity, although those centres are doubtless indispensable for its manifestation. Physiology throws no light on the sublimation of emotional force to higher forms, nor is she concerned in the recognition of two main currents of psychical energy, the one directed outwards to the external world, the other turned inwards and lost in the intricacies of day-dreaming and concentration or internal experience. Similarly devoid of expression in physiological language are our conceptions of psychical dissociation or the splitting up of the unified conscious stream into smaller, more or less independent fragments, and of regression or the backward involuntary path of mental processes to more infantile conditions. Physiology can never describe a mental process with which it is somehow correlated. The day seems to have come when we must give up those diagrammatic schemes of speech, visual, auditory and kinæsthetic memory centres which we have cherished as ways to the higher knowledge, under the mistaken confusion of imagery with meaning and of thought with language, and the erroneous conceptions of separate watertight mental faculties.

Having made this digression, it may be asked what bearing have the ideas of modern psychology upon the manifestations of the workings of the mind of man as an individual member of a group or herd? Modern psychology lays stress on the fact that the efficient factor in the process of psycho-analysis is not the introduction of the repressed experiences into the conscious field, but the overcoming of the resistances to such an endeavour. I have attempted to show that, in respect of man as a member of a group, the resistances or counter-impulses are of environmental origin and owe their strength to the specific sensitiveness of the gregarious mind. Resistances of identical origin are responsible for the formation of the so-called normal or stable type of mind. These resistances are now allowed to come into being haphazard, and while they contain undoubtedly elements of social value and necessary restraints, they are products, not of a courageous recognition of facts, but of fears, prejudices and repressed instinctive impulses, and consolidated by ignorance, indolence and tribal custom. In this environment, man gives play to instinctive impulses but disguises them from the observer and from himself by the multiplicity of the lines of response his mental capacity enables him to take, with the result that his conduct is much less variable and much more open to generalization than is commonly supposed. The root cause of this generalized conduct of man is his gregariousness; he is

intolerant and fearful of solitude; he is more sensitive to the voice of the herd than to any other influence, it can inhibit or stimulate his thought and conduct, it is the source of his moral codes, it can endow him with energy, courage and endurance and can as easily take these away; he is subject to the passions of the pack in his mob violence and the passions of the herd in his panics; he is remarkably susceptible to leadership.

As a social animal, man suffers from the disadvantages of such an animal to a more marked degree perhaps than any other. In physical matters, he owes to his gregariousness the formation of crowded communities with enclosed dwellings and the seriousness of many of his worst diseases. The only other creature which is well known to suffer seriously from disease as a direct consequence of its social habit is the honey bee. In mental affairs, man owes to social habit his resistiveness to new ideas, his submission to tradition and precedent, and his tendency to segregation into classes. In the society of the bee, two characteristics are prominent. They are, an elaborate and exact specialization of the individual, and a complete absorption of the interests of the individual in those of the hive; these qualities seem to be the source of the energy and power of the whole unit and of the superiority of intelligence it possesses over the individual member. In human affairs, combined action is usually less intelligent than individual action, a fact suggesting how far inferior man is, in respect of combination and co-ordination to the bee. The moral homogeneity so apparent in the society of the bee is replaced in man by a segregation into classes or groups which tends to obscure the unity of the nation and often is directly antagonistic to it. It seems as if a given human unit cannot develop the highest homogeneity within itself, unless subject to direct pressure from without. In the absence of the stimulus of war, the size and security of the modern State tends to relaxation among men of the bonds of national unity, the result being a segregation into classes, which form minor herds in which homogeneity is maintained by the external pressure of trade competition and of political or religious differences. This tendency naturally obscures the national value of specialization and gives it a merely local or class significance and in times when there is an urgent need for national homogeneity may prove to be a source of weakness.

The fact that even the gigantic external stimulus of a great war has failed to overcome the forces of social segregation, suggests that in society, as constituted to-day, there is evidence that simple gregariousness in man has been defeated by the disruptive power of his capacity for varied reaction. The meaning of it all is that man needs to take a lesson from the methods of the psycho-analyst, to overcome the resistances originating from group or class environment and to give his instinctive impulses a chance to come out into the open and conscious field. In other words, man must cease to be smug and artificial or insincere. The species is committed to a certain evolutionary path by the inheritance of the instinct

it possesses and man, if he can but realize the direction of that path, with moral code based on altruism, could by true combination, fellowship and homogeneity, render available in co-ordinated action the maximum energy of each individual to the common good. If he fail to do so, there is much to suggest that the situation of society will be more precarious than usually supposed by those who happen to be in charge of its destinies.

XXI.

De te fabula narratur, and the story about you, reader, is to be found in that curious literature called folk-lore, also in folk-art. As usually defined, folk-lore is the study of survivals, but it is concerned with a process or movement, not with an inert mass of cultural fossils. Despite the fact that folk-lore deals largely with the decadent, the process is not one of mere degeneration, because revival no less than survival must be reckoned with, the history of culture testifying to the continuous interplay of old and new forces. Regarded from without, such a process of change may be expressed in terms of form, but regarded from within it is an expression of the human mind, it is change in respect of the values recognized and sought. Whether savage or civilized, human nature is subject to perpetual transformation, or that something is always disappearing while something else is coming into being. This law holds good of the most backward of societies no less than of the most advanced, and it follows that custom survivals are no mere by-products of a late civilization, but form an essential feature of human history taken at any of its successive states and in any of its branches.

Hitherto, the students of folk-lore have done little more than collect a vast mass of facts and records regarding custom survivals. These details are interesting enough, but the detailed account of the whole process of transformation of culture or custom as it takes place among the folk has yet to be written. We want to know exactly how the new gains at the expense of the old, how it partly re-adapts, partly diminishes and dwarfs, and partly destroys. In fact, we need a study of the psychology of the generation, degeneration, and regeneration of culture or custom. It would seem as if only through psychology, applied in the first place to ourselves, shall we be able to understand the real meaning of folk-lore. Take the case of witch-craft; can we not study it among ourselves more effectively than among savages, even though it be written in larger letters on the surface of their lives? The root feeling must be apprehended by experience before its manifestations can be recognized for what they really are, and the root feeling lurks here and now within the breast of every one of us. Folk-lore does not consist of fossils but of living material; the peasant is half-way between ourselves and the savage, and we can understand both only through our understanding ourselves. There are certain steadfast currents of impulse which govern the flow of the tide of human life in all ages and countries, and, if we read rightly the facts and details as presented in folk-

lore, we should see all those facts in the light of our own nature and be able to say, "Yes, that is how it happens to me," with the result that we shall insist, not on the difference between simple peoples and ourselves, but on our likeness, and that not satirically but with sympathy for both.

In this manner, we can no longer regard primitive man as a skeleton in the cupboard of our past, but rather that he is living in each one of us, and not only in the form of original sin or stupidity. If the reader doubt this, let him contemplate some of the recovered forms of folk-art, so inextricably connected with folk-lore of all kinds. A hundred years or less ago, folk-art would have seemed as absurd and valueless as the most absurd primitive superstitions; but now anyone with a sense of art at all respects it in all its forms. Whatever may be said about aboriginal religion, aboriginal sculpture is real sculpture, having virtues we do not find in our public monuments, just as folk-song is real song with virtues not to be found in revues or musical comedy. But folk-art is not an accident, it is a product of the whole folk-mind, and it tells us that that mind is serious, understanding, with values like our own and with a definite power of expression. In those rough artistic efforts man speaks to man; it is not museum curiosity, but intercourse about the deepest things, and we cannot despise those who made them or any other products of their minds. It is as if you had watched a man across the street with contemptuous amusement, as being a mere oddity, and then had suddenly conversed with him and found him full of your own ideas and perhaps with more than your own power of expressing them.

To ignore what we owe to our spiritual ancestry amounts to a denial of the doctrine of development. The whole history of science proves that it is legitimate to leap from a narrow groundwork of facts to the widest generalizations, so long as the complementary verification is thereafter duly performed. Our business then is to complete what our predecessors began. Hence the prime concern of students of primitive culture is to determine how cultural change has proceeded under the joint stress of internal and external influences. We are ex-savages with customs bearing visible traces of our ancient condition; and, further, being indigenous to the culture area we know, we should be aware by sympathy how the drift on the surface answers to deep-moving currents in the social life. After all, evolution stands for vital process only by a euphemism; there is degeneration to be reckoned with and also revival. There never was a time when the interplay of old and new did not go on exactly as it does now, when survival and revival, degeneration and regeneration, pulsate together in the rhythm of social life. It is as necessary to read the present into the past as the past into the present and folk-lore, old-fashioned stuff though it be, belongs to the here and now, and its facts may at any moment renew their youth in the way that many old fashions have. Take the case of a custom that to all appearances is effete, the so-called fossil. It might be judged as without function, and yet why does it linger on? Simply because it has some sort

of value. Possibly, it helps us to maintain a comfortable automatism, and so long as we do this in regard to such things as matter little, we are enabled to concentrate on things that matter much. Thus the antiquated custom, though it seems functionless sociologically, is of value from a psychological point of view, if only because it is restful.

However, it is not in value so much as in change of value that makes folk-lore so interesting, because it indicates a continual process which is ultimately intelligible only in the light of the cultural life as a whole. Whether we think of folk-lore in association with a people having a literature or with one without, a distinction may be drawn between the traditions that severally depend on organized and on unorganized folk memory ; such organization is apparent in the mnemonic exercises of bards and other old-time remembrances, the insistence on verbal exactness in religious and legal formularies and the schooling of the novitiates at the initiation into old-time mysteries or guilds. It is well known that to such organization we owe many of the tales and fables, proverbs, the prognostics, the leechcraft prescription, and other curious odds and ends in current use now, and that they are but the debased product of yesterday's official wisdom. There is also an opposite process or revolution from below of which examples are forthcoming from a study of folk-tales. These work their way up to polite society though not without submitting to a change of garb. The underworld in which they are reared is the nursery. These old-time values retain their spell, we shudder at ogres and long to dance with fairies ; moreover, these values grow up with us and in transmuted form enrich adult life, quickening the sense of wonder, the spirit of adventure, the love of simple and vital things. Similarly, a change of meaning may be caused by the transference of a theme from religion to art. A discarded rite colours an incident in a folk-story, a mask, once of sacred import, decorates the actor in a secular play, a charm against the evil eye becomes an ornament, and so on. Regarded from the psychological standpoint appropriate to the study of value, we may say that what has happened is merely that a new interest or fresh system of meanings has apperceived the theme in question. Now this new interest is the chief connexion between primitive minds and our own. The theme while it was purely religious may have been unintelligible to us, a mere curiosity of the past expressed in a language we do not understand and to be studied only from the outside. But when it is adopted by art, we understand it from the inside ; we see the workings of the artist's mind as being like our own, because now it is presented in terms of beauty, and because the artist has put himself and not merely the savage into it. His voice has changed and become his own and almost our own as his values have become our values. In fact, man's values do not change, however much his rites and ceremonies may do so ; primitive values are our values, however strangely they may express themselves.

Admittedly, the prevailing movement in the underworld of folk-lore is

downhill; that is, first an institution disappears, then its associated belief goes and all that remains is perhaps a floating superstition or some memory of it preserved in story. But we need to remember that beliefs die hard and frequently secretly bide their time that they may recloke themselves later in another form. We know from history, especially our own, that dominant people can be good learners, but the under-folk ever find it hard to forget. Change of meaning or transference from one interest to another on the same plane of culture can sometimes be explained as a process of modernization; thus, old songs are accommodated to new instruments, a mummer's play makes room for a popular hero of the day or unfamiliar animals give way to familiar. A general principle, however, in regard to such causation may be accepted and that is, that within the domain of folk-lore the process of re-adaptation is always subconscious. A breach in the continuity of tradition having somehow occurred, the tissue repairs itself spontaneously, partly by the assimilation of new matter and partly by the coalescence of such elements as survive. Conscious renovation seems to occur only at the higher levels of culture. Folk-beliefs are constantly liable to interference from upper social strata, hence the trend of cultural degeneration is towards a rally of the decadent values under the banner of some impractical interest, such as song or story. At the same time, the æsthetic tradition of the folk tends to be the legatee of all other expiring interest; memory and fancy can still play with thoughts that bear directly no longer on the daily round of work. Some solemn ritual, for instance, is disestablished and descends to the underfolk, deviating from its original meaning as it drops; but the grave of religion is the seed-bed of art. First, the popular imagination adopts some surviving elements, their constructive genius re-adapts the rude material to some high moral theme, whereupon the cycle of change is complete and the downward way compensated by the upward way. Just as bunting is bunting, but the flag a nation's pride and hope, so the old themes embodied in national folk-lore are the golden thread or life-line by which we can trace our way through the maze of the ages and find ourselves awaited at the threshold by our own selves in more primitive garb. They are even more than that, for those same themes may be so re-adapted as to provide a playground for the mind and so save our emotional life from being starved and perverted. In folk-lore is to be found the story of man and ourselves, and despite a certain litter of dead wood there is hidden therein many an ancient heart of oak, awaiting only the spring call of sympathy and understanding to show growth and efflorescence.

XXII.

Having spent my Sunday afternoon in Hyde Park listening to a variety of fervid orators each explaining, according to his lights, the way to better things, be they in respect of economics, politics, sociology or religion, I

have come away wondering whether much that I have heard be truth or fiction. The conclusion I have reached gets me no farther than to think that it requires as much imagination to see things as they are as to fancy the things which are not. The visionary may after all be a realist, and his sight be none the less sight because it is insight or foresight, and what the reformer sees in the present or dreams of the past may do more to determine the future than any literal truth. It would seem also that actual facts have but an indirect or even remote bearing upon the opinion which moulds men's actions, and that there are various media through which facts have to be transmitted before they influence conduct. So with my friends the orators, first there are the facts themselves, about which men seldom agree; then there is the report of the facts which varies with the speaker or reporter; next there are the different impressions which the same facts, and even the same report or speech on them, make upon the recipients; and lastly there are the diverse reactions produced by the impressions in different minds. Put in another way, there is not merely the distinction between the seed and the soil but the sower comes in as well. The net result of it all amounts then to this, that it needs little ingenuity to show that truth and fiction are not entirely incompatible with one another, and that it is probable that I have been listening to as much fiction as truth.

Few people realize what large and beneficent parts fiction plays in human history and progress. The law itself consists to a large extent in legal fictions, and even the British Constitution depends upon its conventions which are a species of fiction unknown to the law. The sovereignties of the Crown and of Parliament and what are called the privileges of the people are nothing but legal fictions built up on the original fiction that the sovereign was the master of fiction. Fiction has ever been a valuable instrument of progress and what reformers could not do by constitutional or legal fictions they have sought to achieve by philosophical, historical or religious imagination. If the reader doubt this, let him refresh his memory by dipping into Bacon's "New Atlantis," or More's "Utopia," or Rousseau's "Contrat Social," or Plato's "Republic." I think he will find that some of those books and most of their modern imitations do not profess to be history, but rather are fictions and intellectual efforts to represent new panaceas as plausible reversions to some more primitive stage of civilization. Even the Bible is not devoid of fiction; so much so that the legend of the fall of man may be regarded as the first step in his ascent. It would not be possible to persuade a man to rise if he were convinced that he had always been flat on the ground and possessed no means to raise himself. From this preliminary it was but a step to convince man that his natural posture was erect and that only his own or other people's sins had brought him where he was. He had had a fall, but he could get up if he chose and regain the paradise he had lost; so from that conception of the fall of man there came the chance of his

recovery. Progress, after that, was but reversion to a former state and a retracing homeward steps; this explains why for many thousands of years the human race has visualized its pioneers as homeward bound. Men could only be persuaded to move forward by the conviction that they were going back, and their leaders shared and encouraged the conviction.

It seems to me that they still do so. Politicians and historians, poets and philosophers all seem to hold up mirrors of Paradise as beacons to mankind. Even Darwin gives colour to the impression that hope depends upon historical fiction. Many still believe in those mediæval legends of liberty which the parliamentarians invented as weapons against the Stuarts, and even the reformers before them appealed to a primitive Christianity and propounded the fiction that error was modern and authority usurpation; and in this our day it is difficult to deny that every fresh movement for reform adds to the stock of historical fiction and implies a reversion to the past. My daily paper and the orators in the park say "what is democracy but a form of government that existed among all primitive peoples? What is the proposal to nationalize the mines and the land but a reversion to the oldest known form of land tenure?" These ideas are all borrowed from a fiction of the past and imply the creation of a social order the exact antithesis of our present one. It is curious how history repeats itself. The eighteenth century was more philosophical than the seventeenth, and its legends took a wider field than national law and history. It revived the classical notions of Nature and fell back on the rights of man. The age of reason rejected the dogmas of the divines, but the rights of man were as original to the philosopher as sin had been to the theologian. Man was born free, not as an individual, but as a genus, and if everywhere he was in chains, the fault was not Satan's or the people's, but that of man's self-deputed governors who had corrupted a perfect human nature. Undoubtedly, the first expression of self-conscious democracy is the hunt for a scapegoat for the sins of which it has inherited the consequences.

For this century, restoration and reaction appear with a more logical plea for reversion. The Middle Ages have receded into a distance which lends enchantment to the view, and a little of romanticism portrays them as a golden age. Knowledge of them is not more popular or more exact than the Reformer's acquaintance with primitive Christianity or Rousseau's with his savage of prehistoric times; but men seem as urgent as ever to escape from the ills they know, and the easiest way is recommended as a retreat to the past of which they are curiously ignorant. We hear much of appeals to reversionary instincts, but we are still too near the Great War to be able to forget that there was much reversion to original type in that four years, and our would-be reformers, by invoking a popular psychology, may arouse a force which they cannot control, and the instinct towards reversion may refuse to stop at the cherished paradise of fiction. The main thing about that paradise is the general idea, and precision is

regarded as a mistake. To attempt to locate historically this communal heaven would be as futile as to send a party from Bagdad to survey the garden of Eden. The truth is, the man who lives in a fog worships a sun he has never seen, and so it is with the bogged, will-o'-the-wisp-pursuing reformers. They need to cultivate the garden of their minds instead of postulating a paradise they cannot cultivate. The practical moral is that the provision of historical training and research for those who can influence and exercise votes is not so much a matter of indifference to the community as is commonly supposed. Both the French and the Russian revolutions broke out because there was little historical sense in the governments or the communities of either country; and while there is more of that commodity in Britain, the orations of my Sunday-afternoon entertainers suggest that we have not much to spare if we want to keep the peace and preserve our social sanity. In the language of Public Health, the epidemic constitution appears to be that of hysteria toppling towards dementia. *Quis Deus vult perdere, prius dementat.*

FARMING IN AUSTRALIA AS A CAREER FOR THE PUBLIC SCHOOL BOY.

By Major-General Sir ROBERT PORTER, K.C.B.

I was fortunate enough on my way home from Australia to secure, at Gibraltar, *The Times Trade Supplement* dealing with Australia and New Zealand. In reading the article by Senator Millen, entitled "On the Horizon," I was glad to see that he had outlined there the immigration policy of the Federal Government, from which it would appear that a serious attempt is about to be made, by the Commonwealth, to attract immigrants of the right sort to Australia.

One of my objects in visiting Australia was to try and find a career for my own boys, and the time I spent in that country was largely taken up by special inquiries as to the advantages offered, particularly for those who intend to go on the land, for boys of the public school life. In the '70's and early '80's many boys of this class went to Australia, took to farming, and to-day one finds many up-country stations owned by them or their descendants. These young men, who went from our public schools and universities at that period have left their impress on the country. They took with them those remarkable habits of character, thought and action so dominant in the English public school boy and have left them as a heritage in every Australian State. One sees the old English home life in most of the up-country stations to-day.

For reasons I need not go into at present, this class apparently ceased to emigrate to Australia. Emigration, however, continued from this country, and in a great measure by assisted passages, Australia got a fair type of our country population, who have made capital settlers.

Australia is an immense continent, with a potential wealth in agriculture, minerals, timber, etc., not surpassed by any country in the world. It has a beautiful climate, temperate in the south and tropical in the north, the latter probably having the greater potential wealth. It has an area equal to the whole of Europe without Russia, and a population of something under 6,000,000.

As I have said, I was mostly interested in the various land settlement schemes, and the opportunities offered for the public school boy. I must say I was very greatly impressed in this direction. I consider the country offers to him a good, clean, manly life in the open air under a most beautiful sky. Nowadays as farming may be regarded as a scientific profession, this kind of life gives to thoughtful men a most delightful and absorbing subject for study and thus does away in a great measure with the *ennui* which formerly existed in up-country stations.

In each of the five Provincial States of the Commonwealth, there is a State Agricultural College, in which students are taught not only the

practical side of the business, but also the scientific branch of farming; chemistry of the soil, fertilization of plants, drought-resisting seeds and grasses, selection of stock, wool classing, all these come into the curriculum.

I had the advantage of spending a day at the Hawkesbury Agricultural College, New South Wales, under the guidance of the principal, Mr. Potts, who, by the bye, is a north countryman, and, if he will allow me to say so, one of the best type of men we have sent to the Antipodes. The session was in full work; there were 200 students, all housed in the residentia part of the college. Most of these boys were Australians, but I saw some New Zealanders, South Africans, one or two English boys, two Japanese and two from Samoa, the latter learning dairying and cheesemaking.

"Thorough" was writ large over the whole place, from the principal's face to the bacteriologist, who, by his research work, sounds the death knell of the various pests that may attack the fruit in the orchard, or the bee in the apiary.

I saw the boys shearing sheep, milking cows, judging cattle for the market, making butter and cheese, classing wool, and mending harness. I also saw other batches in the various classrooms learning the latest scientific problems relative to the breeding of stocks; others working with the microscope trying to elucidate the nature of some new fungoid growth that had recently attacked tomatoes; whilst others in the chemical laboratory were determining the amount of potash needed to be added to certain soils.

Everything relating to farming, whether it be orcharding, sheep or cattle farming, horse or poultry raising, bee culture, or the plucking of ostriches, a bird that was introduced into Australia a few years ago, is taught in the college, and taught with a thoroughness that raised my hopes as to the future of Australia as an agricultural country.

If not transgressing too much on your space, I would like to throw out a hint to the Commonwealth Government or to its representative at Australia House. If boys of the public school type are wanted, let him circularize the heads of the schools of England, informing them what Australia has to offer as inducements for young men of that class to emigrate, in the way of educational facilities at their colleges; settlement on the land; advances for the purchase of stock, etc., and I am sure he will have a good response.

This is the same class of boy that has built up this great empire of ours, and if only given an opportunity, will assist Australia in her present need.

There are numbers of them only waiting for a lead; the public schools are crammed to overflowing, and have long waiting lists to-day.

Formerly the public services, the Navy and Army, offered suitable careers for many boys of this class, but the latter two services are at present in the melting pot, and no one can foresee what the future may

bring forth. The League of Nations may change the whole face of things so far as the fighting services are concerned.

A boy intended for settlement on the land should go out about 16½ or 17, attend if possible the three years' course at one of the Agricultural Colleges, get a Diploma in Agriculture, given after examination. Then let him have a year on a farm according to the particular branch he intends taking up. By that time he will be 20 or 21, he should then have acquired sufficient knowledge and wisdom to look after himself. If he has some capital, so much the better; if not, there are many appointments as managers, etc., suitable for one who has a diploma. The principal of the college told me he had numbers of applications, not only in Australia, but from other countries and islands in the Southern Hemisphere for managers.

The present policy of the Commonwealth Government is to assist settlers, and I have no doubt that policy will be adhered to in the future, so that young settlers on the land will be able, not only to acquire land on favourable terms, but money advances will be given them to buy stock, etc.

In all things earthly there is usually a fly in the ointment. So far as I see, there are two flies in this case. One is drought, and the other the great distance Australia is from England. With regard to the first, which is the serious one: droughts do occur in cycles; however, by irrigation, the sinking of artesian wells and the increase in the railway communications, droughts will not be in the future so devastating in their effects as in the past. The second fly—the distance from England, about 10,000 miles, a six weeks' voyage on a mail steamer, may seem a long distance to a boy going abroad for the first time. But for one who adopts a colonial career, whether the distance be 3,000 or 10,000, does not really matter much. Wherever he has gone, he will not have many opportunities of returning home. In one case his letters and papers will be, say, a fortnight old, in the other case, six weeks, *c'est tout*.

I would like to say, in conclusion, do not let a boy start for Australia, or indeed any other colony, with any illusions that he is going to have a good time and to pick up gold by the wayside. If he goes with these ideas he will be a failure: on the other hand, if he goes out determined to work hard, probably harder than he would have to work at home, Australia offers good opportunities for gaining a competence, even wealth, and a comfortable home in one of the great bulwarks of our Empire.

Clinical and other Notes.

NOTES ON THE APPOINTMENT OF A DEPUTY ASSISTANT DIRECTOR OF MEDICAL SERVICES (SANITARY) OF A DIVISION (OR DISTRICT) IN INDIA.

By Captain R. A. ANDERSON.

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The following notes are not written with the idea of adverse criticism, but with the belief that the present system of military sanitary administration in India could be vastly improved by reorganization.

The writer is not desirous of claiming originality of ideas. The conclusions have been arrived at from experience gained by having held the appointments of Deputy Assistant Director of Medical Services (Sanitary) of a Brigade and three different Divisions during his present tour of duty in India; from comparison with what one hears of the organization at present in existence at home; and from discussions with Assistant Directors of Medical Services and Deputy Assistant Directors of Medical Services (Sanitary) of other divisions (now districts).

To begin by considering the more important duties of a Deputy Assistant Director of Medical Services (Sanitary). They may be briefly summed up as consisting of the keeping of statistics of sick, epidemiology, hygiene and pathology, including bacteriology. It is with regard to pathology, including bacteriology, that one feels that the present system fails. One has yet to meet the District Sanitary Officer who conscientiously asserts that he can perform the duties of a Deputy Assistant Director of Medical Services (Sanitary), particularly in regard to his laboratory, to his own satisfaction. Under the present system not only must the bulk of the laboratory work be left to the assistant surgeon in sub-charge, but also there is an entire absence of co-operation with the clinician.

It is obvious that the duties of a sanitary officer should be concentrated on the prevention of disease and everything pertaining to that end, whereas the duties of the pathologist and bacteriologist should be in intimate association with the clinician, not only as regards the clinical aspect of the case, but also with regard to treatment and results.

It is impossible to combine two essentially different branches of the profession, with efficiency, in one officer.

This has been realized at home, and the two branches separated by the creation of a separate hygiene department and a separate pathological department, with the appointments of directors, assistant directors, and deputy assistant directors, of hygiene and pathology respectively.

If the above has been found necessary at home, where undoubtedly sanitation is modern, surely it is even more necessary in India, where sanitation is as yet primeval, and where a far greater scope of work is afforded to the pathologist and bacteriologist.

One has long waited to see other sanitary officers in India writing on this subject, but presumably like myself they suffer from the natural abhorrence of seeing themselves in print. It is hoped, however, by breaking the ice, others will be stimulated to give their views.

To consider the question from the point of view of (*a*) of the district, and (*b*) of the division in the field.

(*a*) It is essential that there should be in the district a Deputy Assistant Director of Hygiene and a Deputy Assistant Director of Pathology (including Bacteriology). The Deputy Assistant Director of Hygiene with regard to the whole of the district to deal entirely with prevention of disease, sanitation of barracks and cantonments, investigation and control of epidemics and the keeping of statistics of sick. The Deputy Assistant Director of Hygiene though relieved of the laboratory will have plenty of work to keep him busy. It is realized that owing to the present financial stringency and for years to come, there will not be any major sanitary improvements carried out, consequently the work of the Deputy Assistant Director of Hygiene will be concentrated on making the best of the present circumstances.

The Deputy Assistant Director of Pathology should be stationed in the headquarters station of the district. His laboratory should be in close proximity to a hospital. He should work in close co-operation with the clinician, seeing all cases with the medical officer, and thereby giving the medical officers the opportunity of studying the pathological and bacteriological aspect of their cases.

Similarly, one believes the other specialists should see cases with the medical officer instead of the cases being sent to the specialist, because by this system the medical officer becomes gradually divorced from his profession by specialisms, and it will lead to the habit of unnecessarily sending cases round the different specialists, not entirely through the fault of the medical officer, as he does not get the chance of learning the specialist point of view for himself.

The Deputy Assistant Director of Pathology should likewise proceed to out-stations and see cases in other hospitals, taking material for cultural work, etc., where necessary. He should further work in co-operation with the Deputy Assistant Director of Hygiene with regard to epidemiology, etc.

(*b*) The same principle should apply to a division in the field, the Deputy-Assistant Director of Pathology going out with his mobile and completely staffed laboratory to an advanced base.

On the command staff the present Deputy Assistant Director of Medical Services (Sanitary) should be re-named an Assistant Director of Hygiene. An Assistant Director of Pathology is not required, and the appointment of one does not appear to be warranted.

The Assistant Director of Hygiene of the command could materially assist the Deputy Assistant Directors of Hygiene of districts, by being acquainted with the most urgent sanitary problems in each district, and by keeping the different Deputy Assistant Directors of Hygiene informed of the occurrence of infectious diseases, not only in other commands but in the different districts in India. This information is of the greatest use to the Deputy Assistant Directors of Hygiene as regards movements of troops.

There is nothing more disheartening to the sanitary officer of a district or a division in the field than to find every one of his schemes for sanitary improve-

ments turned down and a system of temporary patchwork instituted instead. The Assistant Director of Hygiene of the command should use his influence by putting the more important sanitary schemes in his command up to General Headquarters (through, of course, the Deputy Director of Medical Services and "Q" Branch), and keeping the Deputy Assistant Directors of Hygiene informed when these different schemes will be taken up.

To further consider the scheme at Headquarters: There should be a Director of Pathology (including Bacteriology) and a Director of Hygiene.

The Director of Pathology should have a large central laboratory equipped to modern standards and one or two Assistant Directors of Pathology. The laboratory should be able to supply stock vaccines, stock cultures, and standard agglutinating sera to the Deputy Assistant Directors of Pathology. There should be every facility for research work.

The Director of Hygiene would have duties practically similar to those of the former Assistant Director of Medical Services (Sanitary) at Headquarters with the exclusion of all matters pertaining to pathology and bacteriology. He should be acquainted with military sanitary problems in India, and should be able to see them for himself, so that he could advise the Director of Medical Services on questions referred to him by "Q" Branch.

I am indebted to Colonel A. E. C. Keble, C.B., C.M.G., D.S.O., Assistant Director of Medical Services, Peshawar District, and to Lieutenant-Colonel H. M. Babington, D.S.O., R.A.M.C., Senior Medical Officer, Peshawar, Specialist in Pathology and Bacteriology (Millbank), for their advice and views.

ENTOMOLOGICAL NOTES

By Major J. E. M. BOYD, M.C., F.E.S.

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In recent years, the louse has attained some notoriety owing to the amount of inefficiency caused by it, in the late war, amongst troops on all fronts, so it is thought that a short historical record may prove of interest.

The earliest record of this insect is contained in the Ebers or Leipzig papyrus of the sixteenth century B.C., which deals with the destruction of fleas and lice. Linnæus (1758) just makes a brief mention of them.

Husemann (1867) records phthiriasis from the middle ages.

Fischer (1915) gives quotations from writers of the sixteenth century, A.D.

Goldsmith's "Animated Nature" (1779), vol vii, p. 274, gives a short account, of which the following is an extract:—

"The phthiriasis or lousy disease, though very little known at present, was frequent enough amongst the ancients. Herod, Antiochus, Epiphanus, Alcenan the poet, Pherecydes, Cassander, Callisthenes and Sylla all died of this disorder." He gives no authority for this statement, which may or may not be true, though the ancients were without doubt lousy.

Buffon (1823), in his "Natural History," also makes mention of the louse, but his account is evidently pirated from that of Goldsmith. Both he and the latter record the popular belief that a louse could become a grandfather in forty-eight hours.

In another old volume, in the writer's possession, from which the title page has unfortunately been lost, so that both the date of publication and the name of the author cannot be traced, the following account may be found:—

"This peculiar disease was more frequent among the ancients than it is with us, which was owing to their using hardly any linen cloth next to their skin; their woollen toga, mantles and other coverings, which were but now and then sent to the scourer, harboured the vermin much more snugly than our linen garments."

Pherecydes, a philosopher of Sycos, who had the honour of being the master of Pythagoras, declared in a letter, which he wrote a few days before his death, 'that he was covered with lice,' and did not seem to show any reluctance at mentioning it." This author appears to have known a little more about the life history of the louse than either Goldsmith or Buffon, as he gives the life cycle as eighteen days. Kirby and Spence (1828) in their book "An Introduction to Entomology," vol. i, p. 83, state: "It is a vulgar notion, that a louse in twenty-four hours may see two generations; but this is rather overshooting the mark. Leeuwenhoek whose love for science overcame the nausea that such creatures are apt to excite, proves that their nits or eggs are not hatched till the eighth day after they are laid, and that they do not themselves commence laying before they are a month old. He ascertained, however, that a single female louse may in eight weeks witness the birth of 5,000 descendants."

These same two writers also state that "no monarch was ever suspected of imposing a tribute of lice upon his subjects. Yet we are gravely told that in Mexico and Peru such a poll-tax was exacted, and that bags full of these treasures were found in the pa ace of Montezuma (Bingley, "Anim. Biogr.," first edition, iii, 437)." They also mention that Pheretima, Antiochus, Epiphanus, the Dictator Sylla and the two Herods, the Emperor Maximin, and the great persecutor of the Protestants, Philip the Second, were carried off by phthiriasis.

It was also considered by these two writers that the term "phthiriasis" may have included not only diseases due to lice but also to acari and larvæ of sorts. To quote from their book: "the louse feeds on the surface of the skin," and that "the nits or eggs are deposited on the small hairs of the skin, the animals being found on the skin or on the linen, and not under the cuticle as some authors have represented."

If these observations be allowed their due weight, it will follow, that a disease produced by animals residing under the cuticle cannot be a true phthiriasis, and therefore the death of Alcman the poet, and of Pherecydes Syrius the philosopher, must have been occasioned by some other kind of insect; for speaking of the lice, to which Aristotle attributes these catastrophes, he says, "they are produced in the flesh in small pustule-like tumours, which have no pus and from which, when punctured they issue."

Dr. Willan has quoted, with approbation, two cases from *Amatus lusitanus*, which he seems to think correctly described as phthiriasis. In one of them, however, which ended fatally, the circumstances seem rather hyperbolically

stated—where it is said that two black servants had no other employment than carrying baskets full of these insects to the sea.

Dr. Mead, from the German Ephemenides, gives an account of a woman suckling her child, from whose breasts proceeded very minute vermicules (Media Sacra, 104-105). He also mentions an insect, a native of America, under the name of *Pediculus ricinoides*, which upon the authority of Rolander, gets into the feet of people as they walk, sucks into their blood, oviposits in them, and so occasions very dangerous ulcers. It would be an acarus, he observed, but it has only six legs.

When Thomas á Beckett was murdered (A.D. 1170), it was found, on the removal of his hair shirt that the inside was shimmering with the eggs of lice; this fact added to his holiness in the eyes of the populace, as it showed that he had worn the garment almost continuously.

Robert Burns wrote a poem between 1759 and 1796:—

"To a louse, on seeing it on a lady's bonnet in church." He also mentions the louse in "The Merry Muses," privately printed in 1827; only ninety-nine copies of this work were printed and it is not the kind of book usually found in a lady's drawing-room.

References to published articles may be found in the "Index Catalogue of the library of the Surgeon General's Office, United States Army, first and second series," a very valuable book of reference for all medical and surgical publications.

The best recent bibliography on the subject is to be found in "Parasitology," vol. x, pp. 1-42, and 582-586, by Professor Nuttall, F.R.S. It is almost impossible to quote any references which are not contained in these two articles.

The *Lancet*, 1901, vol. ii, p. 819, records a case of a child being killed by the complications following infection with lice. Andrew Murray, in 1861, read a paper to the Royal Society of Edinburgh, in which he describes the various colorations of lice, found on the several races of mankind; a few of these may be mentioned: Negroes, black; Indian, dark and smoky; Japanese, yellowish brown; Africander, orange; European, light.

He does not however consider them to be of different species, any more than the races of mankind, who are all looked upon as members of the human race, the differences being equally slight in man and louse.

For recent information on these insects, reference may be made to "Entomology for Medical Officers," by Colonel Alcock, C.I.E.; "A Text-book of Medical Entomology," by Patton and Cragg of the Indian Medical Service; and to the JOURNAL OF THE ROYAL ARMY MEDICAL CORPS.

A VILLOUS GROWTH OF THE BLADDER.

BY CAPTAIN J. H. M. FROBISHER.
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The following case is sufficiently uncommon in the service to be of interest.

Dr. W., R.F.A., was admitted to the Royal Herbert Hospital complaining of passing blood in the urine. He had passed a certain amount when in barracks and had at once reported sick. He again passed a large quantity of blood in the reception room. This was seen by a medical officer. He complained of no pain and he stated that he had never passed any blood previously.

He was sent to bed and the following morning was cystoscoped. This examination revealed a villous growth of the bladder lying to one side of the left ureter. Both ureters were secreting normally. He had no further bleeding whilst in hospital. Three days later he was operated on. After anæsthesia with ether, the bladder was washed out and distended with boric lotion. A supra-pubic cystotomy was next performed and the bladder slung up with two stout silk sutures. With the aid of retractors the tumour could be seen lying in the position shown by the cystoscope. An assistant next put a finger in the rectum and pushed the bladder forward. This however did not assist in making the tumour more get-at-able and was given up. An electric head lamp was used to give a good illumination. With some difficulty a purse-string suture was placed round the base of the growth, and this was then cut away. The base was next cauterized freely, and the suture tightened. Very little bleeding took place. The bladder seemed perfectly healthy and it was decided to close it completely. This was done in the usual manner by Lembert sutures.

The after history was uneventful. The bladder and skin wound healed by first intention. The patient was only catheterized twice and no catheter was tied in. A few blood clots were passed at first but the urine was clear on the second day. It was fortunate that the growth did not invade the ureteral orifice, as this would have necessitated transplantation of the ureter, and very considerably added to the difficulties of the operation.

I have to thank Lieutenant-Coloned R. Tilbury Brown, C.M.G., D.S.O., R.A.M.C., for his permission to publish this case.

Lecture.

ANOXÆMIA AND ITS EFFECTS AS SEEN IN GAS POISONING.[1]

By C. G. DOUGLAS, C.M.G., M.C., D.M.

By the term "anoxæmia" is implied a condition in which the rate of supply of oxygen by the arterial blood is insufficient for the requirements of the tissues of the body. Such an insufficiency may arise from a variety of causes: deficiency of oxygen in the atmosphere, interference with the free gaseous interchange between the blood and the air in the lungs, a diminution in the capacity of the blood to transport oxygen in the normal quantity (no matter whether that alteration is due to circulatory failure, to excessive hæmorrhage, to abnormal destruction of red blood corpuscles within the body or to anæmia of some other origin, or to some factor which so alters the hæmoglobin in the blood that it is no longer capable of combining with the normal proportion of oxygen even when there is no impediment to the entrance of oxygen from the air in the lungs into the blood), or lastly any cause that may hinder the free liberation of oxygen from the oxyhæmoglobin in the red corpuscles as the blood traverses the capillaries in the tissues.

[1] A lecture delivered at the Royal Army Medical College, February 9, 1921.

The almost instantaneous loss of consciousness that occurs when we breathe an atmosphere devoid of oxygen—the mental failure, loss of muscular power and co-ordination, and the weakening of the heart's action which ensue in even normal people when subjected experimentally to atmospheres in which the oxygen concentration is seriously lowered—the headache, nausea, weakness and misery which result from more prolonged exposure to atmospheres moderately impoverished of oxygen, a train of symptoms which we meet with in mountain sickness—all these phenomena must bring home to us the paramount influence on the cells of the body of a proper supply of oxygen. We can understand, too, that anoxæmia, if it should develop at a time when the body is already affected by some gross pathological change, may well constitute a factor of overwhelming importance.

In this lecture I am going to confine myself to the question of the extent to which anoxæmia may be a direct or contributory cause of serious symptoms, if not of death, in cases of gas poisoning which may occur during military operations, and I am naturally led to speak first of all about the effects produced by those poisonous gases which were deliberately used for offensive purposes and figured so largely during the recent war.

As you are aware a great number of different toxic gases were used both by the Germans and by the Allies, but we can fortunately dismiss some of these at once, since, whatever their military value as harassing or neutralizing agents, they did not in practice produce a type of effect that would cause deficiency of oxygen to manifest itself in the body. This statement holds good for the pure lachrymator substances such as xylyl bromide used by the Germans in their earlier type of gas shell, or ethyl iodo-acetate which was used by the British, or the chlorarsine compounds contained in the German Blue Cross shell. It is true that these substances are capable of causing pulmonary lesions of the most serious type, provided that they occur in relatively high concentration in the atmosphere breathed, a fact which has been proved experimentally on animals, but their boiling point is so high that the requisite concentration to produce this effect was never actually attained in the field, and their action was therefore limited to profound sensory irritation of the eyes, upper respiratory passages, etc., an effect which was quite sufficient to disable the opposing troops temporarily. Hydrocyanic acid, which was extensively used by the French and ourselves in gas shell, need not detain us, for this gas does not lead to anoxæmia in the strict sense. It is a true tissue poison, and the vital activity of the cells and organs of the body may be abolished even though the oxygen carrying power of the blood is unimpaired: the tissue cells simply cease to make use of the oxygen that is carried by the blood, the immediate cause of death being failure of the respiration and of the heart.

We are left, therefore, with the so-called acute lung irritant gases, of which the best known are chlorine, phosgene, trichlormethyl-chloroformate, chloropicrin and nitrous fumes,[1] and the remarkable vesicating agent mustard gas or

[1] Though nitrous fumes were not responsible for any serious casualties during the warfare on land, they offered a problem of considerable importance during naval warfare owing to the liability of the propellant charges of guns to get set on fire during action. See in this connexion Surgeon-Lieutenant Fairlie: "Poisoning by Nitrous Fumes," Naval Medical History of the War, *Journal of the Royal Naval Medical Service*, vol. vi, p. 66, 1920.

dichlorethyl-sulphide. I propose to deal first of all with the acute lung-irritant gases.

The most dangerous symptoms which arise in poisoning by the acute lung irritants are due to the onset of acute pulmonary œdema, for the exudation into the alveoli, if considerable in quantity, is capable of causing death by asphyxiation just as certainly as does drowning. The concentration in the atmosphere breathed which is necessary to cause a serious degree of pulmonary œdema differs with the different gases of the group, phosgene, trichlormethyl-chloroformate and chloropicrin exhibitlng a greater toxicity in this respect than chlorine or nitrous fumes. In addition, however, to the effects that these gases produce in the alveoli of the lungs they are liable to exert a marked irritant action on the mucous membrane of the trachea and bronchial tubes, which may be rendered evident by congestion, necrosis of the epithelium and formation of inflammatory exudate. This inflammatory action on the mucous membrane of the air tubes varies in prominence according to the particular gas to which the person has been subjected. Thus phosgene may induce a sufficient degree of pulmonary œdema to cause death without any very definite changes being produced in the mucous membrane of the air tubes, save perhaps in the smallest bronchioles. On the other hand the pulmonary œdema resulting from exposure to chlorine or chloropicrin is always accompanied by very definite inflammatory changes in this situation. This distinction appears to correspond roughly with the differences of sensory irritant power of the different gases, that is with their power to cause pain, lachrymation and violent coughing.

A great deal of information is now available regarding the actual pathological changes which result in the lungs from the action of the acute lung-irritant gases. The main changes can of course be determined by an examination of the organs at autopsies on fatal cases of poisoning in the case of man, and the full sequence of events has been exhaustively studied in experiments on animals. For the full details of these experimental results, I will refer you more particularly to the accounts published by Edkins and Tweedy [1] and by Dunn [2] in England, and to a recent description of the investigations made in the United States which has been edited by Winternitz [3]. You are doubtless conversant with these pathological changes, and I need not here do more than summarize the main features briefly.

A period of delay intervenes before any definite alteration makes its appearance in the lungs. The first sign of pulmonary œdema that can be recognized is an accumulation of fluid in the interstitial tissue of the lungs accompanied by a marked distension of the lymphatics, and this is quickly followed by the appearance of œdema fluid in the alveoli, which can at this stage be recognized practically universally throughout the lungs. In the course of the next few hours the alveolar œdema extends and becomes more intense. As time goes on the dis-

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[1] Edkins and Tweedy: Report No. 2 of the Chemical Warfare Medical Committee published by the Medical Research Council, April, 1918.

[2] Dunn: Report No. 9 of ditto, published September, 1918; Report No. 20 of ditto, published March, 1920.

[3] Winternitz: "Pathology of War Gas Poisoning," Yale University Press, 1920.

tribution of the œdema fluid in the alveoli becomes irregular. Some parts of the lung, mainly those from which drainage by the lymphatics would appear to be difficult, exhibit a solid œdema, the alveoli being filled with fluid to the complete exclusion of air, whilst the œdema in other areas may be comparatively slight. Yet even in the latter case there is a great tendency for the fluid to lie against the alveolar walls hindering access of air to them. The degree of change which may occur in the mucous membrane of the bronchial tubes is very variable. Sometimes, as may be the case in phosgene poisoning, the damage is comparatively trivial and confined to the terminal bronchioles, though even in these cases the bronchioles leading to the lobules of the lung where the œdema is solid may become blocked with exudation and epithelial débris. Such damage is far more striking after poisoning with chloropicrin or chlorine, and affects in the main the mucous membrane of the small and medium air tubes, though even the larger air tubes and trachea may be seriously affected. In these cases the desquamated epithelial cells and coagulated exudate may form casts which seriously obstruct the entrance of air into the alveoli beyond, the obstruction being further accentuated by the engorged and œdematous condition of the submucosa. The capillary network between the alveoli may also be damaged by the gas, evidence of this damage being afforded by thrombosis in the alveolar capillaries in the neighbourhood of the entrance of a bronchiole into a lobule of the lung, though the capillaries in the more peripheral part of the lobule may appear intact. The more severe the dose of gas the more extensive is the capillary thrombosis. The American observers have drawn attention to the possible obstruction of capillaries from compression by strands of fibrin deposited in or upon the alveolar walls. As the inflammatory changes develop in the lungs the even expansion of the lungs by the respiratory movements must be greatly interfered with. Complete or partial obstruction of the bronchial tubes by debris and exudate, and the flooding of alveoli with œdema fluid will greatly impede the entrance of air into some parts of the lung, while the remaining parts of the lung into which air can penetrate will become overdistended, and the weakened alveolar walls may be actually torn by the increased breathing movements or bouts of coughing. Acute or disruptive emphysema will result from this, and as one might expect this condition is most pronounced in those cases which exhibit the greatest degree of obstruction in the bronchial tubes. In the case of pure phosgene poisoning acute emphysematous changes in the lung may play but a small part, though such changes are always prominent when severe poisoning is produced by either chloropicrin or chlorine. During the war different lung-irritant gases were often used simultaneously by the Germans, particularly in gas shell, and the liability of the casualties to show emphysematous changes varied considerably. Pathological changes in other parts of the body than the respiratory apparatus are of an indefinite character after exposure to the lung-irritant gases, and appear to be secondary to the asphyxial condition which ensues from the pulmonary lesions.

You will see that the essential lesions are the gross exudation of fluid into the alveoli of the lungs, inflammatory and necrotic changes in the mucous membrane of the bronchial tubes, acute emphysema, and capillary thrombosis or obstruction. What we have evidently to consider are the precise effects that will be produced by these various changes, for that knowledge is essential if we are to adopt the right lines of treatment.

It is of course apparent that changes in the lungs of the type I have indicated must seriously interfere with the respiratory exchange between the blood and the air, but we must consider in greater detail how far we are to attribute the symptoms to hindrance of oxygen absorption and how far to hindrance of excretion of carbon dioxide. This aspect of the question has been studied extensively by Haldane, and he has dealt with it in a lecture given before this College in October, 1919.[1] I need not therefore do more than recapitulate the main points in his argument.

The interposition of a layer of œdema fluid between the alveolar epithelium and the air in the lungs must of necessity offer a hindrance to the entrance of oxygen into the blood and the escape of carbon dioxide from the blood. Carbon dioxide is, however, far more soluble in water than is oxygen, and it will therefore pass far more easily through the alveolar epithelium and the overlying œdema liquid than will oxygen for any given difference of the partial pressures of the gas in the blood and in the alveolar air. Any increase in the breathing will cause a material lowering of the partial pressure of carbon dioxide in the alveolar air and so facilitate the escape of this gas from the blood, but the small proportional increase in the partial pressure of oxygen in the alveolar air will have little effect in promoting the entrance of oxygen into the blood. Provided therefore that the accumulation of œdema fluid in the alveoli is the only factor that we have to reckon with we can see that it might be possible for the patient to exhibi symptoms of profound deficiency of oxygen without affording any evidence of material retention of carbon dioxide.

If, however, in addition to the development of œdema the structure of the lung becomes disorganized by the development of areas of acute emphysema and of partial collapse, the proper distribution of air through the lung at each breath will be upset. The areas of partial collapse will be ill-ventilated, and though the emphysematous areas may be over-ventilated, this over-ventilation may be unable to compensate for the deficiency of ventilation in the collapsed areas owing to the relatively small proportion of alveolar wall and capillaries to the air contained in the emphysematous spaces. The net result will be that the carbon dioxide concentration in the alveolar air, and therefore in the arterial blood, will become higher than normal; we shall, in fact, have a condition in which severe deficiency of oxygen is accompanied by some degree of carbon dioxide retention.

In both these cases, however, it is the hindrance to the oxygen intake that is responsible for the really serious effects on the body. Accumulation of a certain excess of carbon dioxide does not cause nearly so widespread or disastrous results as profound shortage of oxygen. Moreover, the great sensitiveness of the respiratory centre to any rise of carbon-dioxide pressure in the blood implies that accumulation of carbon dioxide will be accompanied by great hyperpnœa, which will lead to a corresponding increase in the rate of elimination of carbon dioxide. Deficiency of oxygen, if accompanied by increase of carbon dioxide concentration, can, it is true, stimulate the respiratory centre; but such an effect is but slight when compared with the action of carbon dioxide.

The question now arises as to the effect that will be produced by capillary

[1] JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, December, 1919. See also Haldane, *British Medical Journal*, February 10. 1917.

thrombosis or vascular obstruction in the lungs. If these effects are very extensive and severe, as might well be the case in the most severe type of gas casualty who succumbs very quickly after exposure, we can see that they might become a very important accessory factor in causing death, but Dunn's observations on animals have shown clearly that death may ensue when the thrombosis is limited to but certain parts of the lobules, and the fact that the cyanosis in the majority of even serious gas casualties can be relieved by the administration of oxygen points to the fact that in such cases the shortage of oxygen arises from the hindrance to the entrance of this gas into the blood owing to the presence of the fluid in the alveoli, rather than to capillary obstruction, and at the same time suggests that there can be no material passage of venous blood through the non-aerated portions of the lung where the alveoli are completely filled with œdema fluid. The nature and significance of the vascular changes that can be observed in the lungs of animals after exposure to lung-irritant gases have been critically discussed by Barcroft in a lecture given at this College in October, 1919[1], and he makes it clear that the circulation through the non-aerated parts of the lungs is shut down—at least while the animal is resting.

In this connexion I would call your attention to what one sees in cases of ordinary lobar pneumonia. As you know, a patient may have a considerable part of his lungs consolidated, and yet exhibit little or no cyanosis. Were the circulation still continuing freely through the hepatized parts, where the alveoli are so blocked with exudate that no air can enter them, we should expect the blood leaving the lungs to have a much lower oxygen saturation than normal, seeing that part of it would have passed through aerated parts of the lungs, and part would have had no opportunity of gaining oxygen, and we should therefore expect cyanosis to be pronounced. Actually it would appear that in such a case there is an almost complete cessation of the circulation through the consolidated areas, and that the aerated portion of the lungs is practically sufficient to provide for the patient's oxygen requirements during rest (it must be remembered that the normal lungs are capable of meeting the demands of the body even though the rate of oxygen consumption rises to ten times the resting figure, a value which can easily be reached in severe muscular work).

By the kindness of Professor Meakins, of Edinburgh, I am able to show you X-ray photographs of the lungs from a case of lobar pneumonia, in which the blood vessels have been injected with an opaque preparation; and you will see that though the circulation through the lobe which is in the stage of red hepatization appears to be only moderately diminished, there is a general lack of the injection in the blood-vessels in the lobe in the stage of grey hepatization, only a few of the larger vessels being apparent in this region.[2]

Haldane[3] has pointed out that when severe deficiency of oxygen is accompanied by an increase in the concentration of carbon dioxide in the blood, there is reason to believe that the capillaries in the tissues will be dilated and the flow of blood to the veins increased. The heart will therefore be called upon to pass

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[1] Journal of the Royal Army Medical Corps, February, 1920.

[2] Cp. Meakins: *Archives of Internal Medicine;* vol. xxv, 1920; Gross: *Canadian Med. Assoc. Journ.*, vol. ix, p. 632, 1919.

[3] Loc. cit.: also *British Med. Journ.*, July 19, 1919.

on blood to the arteries at a faster rate, but in the absence of a proper supply of oxygen it may fail to meet the strain, and dilatation of the right side of the heart and venous engorgement will give evidence of this. Owing to the dilatation of the capillaries, the lips and face will exhibit a full blue or plum-coloured cyanosis. When, however, the shortage of oxygen is unaccompanied by retention of carbon dioxide, there will be no capillary or venous engorgement, but the grey ashen colour of the patient's face and the obvious indications of circulatory failure will bear witness to the severity of the anoxæmia.

What we have to face therefore in the hours which follow exposure to the acute lung-irritant gases is the risk of death from sheer deprivation of oxygen. Any retention of carbon dioxide which we are likely to encounter in these cases need not cause us misgiving: in fact it is an actual advantage if the deficiency of oxygen is accompanied by some degree of carbon dioxide retention, for this will have a beneficial effect on both respiration and circulation, and will in addition ensure that there is no diminution in the ease of liberation of oxygen from the hæmoglobin as the blood passes through the tissues, since an increase in the pressure of carbon dioxide in the blood (or more strictly the increase in the hydrogen ion concentration to which this gives rise) causes oxygen to dissociate from oxyhæmoglobin at higher partial pressures. One may note in this connexion that the cases showing congested cyanosis, i.e., those cases in which the deficiency of oxygen was probably associated with retention of carbon dioxide, were not in practice so grave as those which were distinguished by ashen pallor.

This acute stage when death from asphyxia is imminent is practically limited to forty-eight hours or so, in fact some eighty per cent of the deaths from acute lung-irritant gases occurred in France in the first twenty-four hours. When once the critical period is safely passed the chances of ultimate recovery are good. Experience both with men and animals shows that reabsorption of the fluid from the lungs is rapid, and that the structural damage of the tissues of the lungs and of the capillary vessels may be healed to a surprising extent. Though emphysema may remain in some cases it is remarkable how complete recovery may be in the majority. Numerous instances are on record in which men who were on the border line of death in the acute stage returned to combatant service in one or other of the various theatres of the war after the lapse of some months. In our experience during the war bronchopneumonia was an uncommon complication amongst those who survived. Convalescence was sometimes delayed by a train of symptoms in which neurasthenia and a mild degree of anoxæmia dependent on an unnaturally rapid and shallow type of breathing played a part, but I do not intend to enter into this aspect of the question as it has been dealt with by Haldane in the lecture to which I have alluded.

The immediate aim of treatment of these cases will be obvious. What we have to do is to try to tide the patient over the acute period in order to give time for the natural recuperative processes to come into play. We cannot of course rectify the damage that has been done in the lungs, nor can we bring about the immediate reabsorption of the œdema fluid, but we can take steps to combat the anoxæmia. Clearly we must prevent the patient from undertaking any muscular exertion, for if the portion of the œdematous lung which is still functioning is inadequate to transmit oxygen to the blood at a fast enough rate when the patient is at rest and his oxygen requirements minimal, ny wilful increase in the oxygen

consumption of the tissues can only spell disaster. Let me remind you that if one stands up one's oxygen consumption is considerably above the figure obtained when one is lying down, whilst walking at only two miles per hour will raise the oxygen consumption to between twice and thrice the lying value. We ought too to keep the patient warm not only to combat shock but to prevent any shivering or undue increase of muscular tone which might occur if the body temperature tended to fall and would entail an increase in the oxygen consumption in the muscles concerned.

The next thing we can do is to try to increase the rate of transmission of oxygen through the œdema fluid and the pulmonary epithelium into the blood. The rate of diffusion of a given gas through a layer of liquid is determined by the solubility of the gas in the liquid and by the difference in the partial pressure of this gas on the two sides of the liquid, and we can easily raise the partial pressure of oxygen in the alveolar air by giving the patient oxygen to breathe. The only difficulty arises in the selection of the best method of administration of oxygen. Some methods are hopelessly inadequate, others entail the use of so much oxygen that it may prove impossible to keep up the supply of the gas. What we have to aim at is to raise the concentration of oxygen in the air in the lungs to such a point that the gas will traverse the œdema liquid and the pulmonary epithelium at a rate sufficient to saturate the hæmoglobin to the normal extent as the blood passes through the pulmonary capillaries: the relief of the cyanosis will indicate when we have attained this concentration. There is no need to raise the concentration of oxygen in the air in the lungs above the value which is found just sufficient to relieve the cyanosis, for the hæmoglobin is now as saturated with oxygen as it is during normal life, and any further increase in the partial pressure of oxygen only implies a very trifling gain in the amount of oxygen held in simple solution in the blood. As a rule a quite moderate addition of oxygen to the air breathed by the patient will suffice.

In the earlier days in France the nitrous oxide anæsthesia apparatus was employed for oxygen administration. This was certainly effective, but the fact that pure oxygen was used demanded a supply of the gas that could hardly be kept up, and a further disadvantage lay in the fact that pure oxygen if administered continuously for any length of time was known to have an irritant effect on even normal lungs[1]. A great improvement was introduced by Stokes in August, 1916, when he adopted the simple method of allowing a small stream of oxygen to pass through a soft rubber catheter which was introduced through the nose of the patient so that the open extremity lay in the nasopharynx. It was easy by this means to treat a number of cases simultaneously. About the same time Haldane introduced his apparatus for the administration of oxygen[2], and later on this was supplied in considerable numbers to the medical units in France. The advantage of the Haldane apparatus is that oxygen only passes to the patient during inspiration and none of the gas is wasted during the patient's expiration, while the delivery of oxygen can easily be adjusted so that it is kept continuously at the rate necessary just to relieve the cyanosis. If I am sitting still under normal

[1] Lorrain Smith: *Journ. Physiol.*, vol. xxiv, p. 19, 1898.

[2] Report No. 10 of Chemical Welfare Medical Committee, published by the Medical Research Council, October, 1918.

circumstances my alveolar air contains about fourteen per cent of oxygen when I am breathing ordinary air, and if under the same circumstances I administer oxygen to myself by the Haldane apparatus at a rate of two litres per minute the percentage of oxygen in my alveolar air rises to about thirty-four per cent; that small addition of oxygen to the inspired air has in fact sufficed more than to double the concentration of oxygen in my alveolar air. Under these circumstances I should only be breathing about eight litres of air per minute. A gassed case would of course be breathing a good deal more owing to his hyperpnœa, and a greater rate of oxygen flow would be necessary to attain the same concentration of oxygen in his alveolar air. Very often, however, the cyanosis of such cases can be relieved by the administration of oxygen by the Haldane apparatus at the rate of three litres per minute, and it is not often that one has to exceed a rate of oxygen flow of five litres per minute.

The relief of the cyanosis is in itself a clear indication that the hæmoglobin of the blood is being properly oxygenated, but Meakins has recently determined by accurate analysis the effects of oxygen administration in various patients suffering from pulmonary complaints who exhibited signs of anoxæmia accompanied by cyanosis[1]. Both Stadie[2] and Meakins have found that the hæmoglobin of the arterial blood of normal persons obtained by direct puncture of the radial artery is about ninety-five per cent saturated with oxygen, and that in pneumonia accompanied by cyanosis there is a definite reduction of the oxygen saturation of the hæmoglobin in the arterial blood. In a case of lobar pneumonia accompanied by a moderate degree of cyanosis Meakins found that the arterial blood was from eighty-two to eighty-five per cent saturated with oxygen; administration of oxygen by the Haldane apparatus at the rate of two litres per minute raised the saturation of the hæmoglobin to ninety-one per cent, and at the rate of three litres per minute to ninety-seven per cent, a value actually in excess of the normal.

A case of acute pulmonary œdema from gas poisoning will exhibit cyanosis for several days. The cyanosis is of course most marked during the first forty-eight hours and then lessens progressively as the œdema fluid is absorbed and the functional capacity of the lungs regained. The cyanosis is an indication of shortage of oxygen, and long continued shortage of oxygen acts prejudicially on the body. Obviously then our aim should be to keep the cyanosis relieved, and this implies continuous oxygen administration over several days. It is therefore desirable that we should use some method of administration which allows the oxygen to be used with the greatest economy. In practice one can however safely intermit the oxygen supply for short periods; this will often prove a relief to the patient, and will admit of giving him nourishment and of attending to him.

To show how one may proceed in these cases I may quote to you two cases that I treated just after the first Haldane apparatus had been sent to France for trial. The first patient had been gassed twenty-nine hours previously during a gas trench mortar bombardment, and when I first saw him he was semi-

[1] Meakins: *Brit. Med. Journ.*, March 6, 1920: Meakins and Davies: *Journ. of Path. and Bact.*, vol. xxiii., p. 451, 1920: Meakins: *ibid.*, vol. xxiv., p. 79, 1921.

[2] Stadie: *Journ. of Exper. Med.*, vol. xxx, p. 215, 1919.

comatose, with face pale and leaden coloured, and speech incoherent. His pulse was 130-140 in rate, and very weak and irregular, and his respirations over fifty per minute and fairly deep. Administration of four litres of oxygen per minute by the Haldane apparatus improved his colour greatly, and with five litres per minute the colour of his lips and face became almost normal: we therefore kept up the oxygen administration at the latter rate with intermissions of ten minutes every half hour. Beside the change in colour there was a remarkable improvement in the pulse. Within a minute or two of starting the oxygen the pulse rate fell to 120 and became regular and of much better tension, but whenever we left off the oxygen administration both colour and pulse quickly deteriorated. After three hours' treatment with oxygen the respirations had fallen to forty-four and seemed less deep, and the patient was becoming more sensible. We continued the oxygen administration, and after a further twenty-four hours the patient's general condition showed distinct improvement, and he was far more conscious. Respirations were still forty-four but the hyperpnœa was distinctly less; the pulse was 120 in rate and regular and strong. Cyanosis returned on stopping the oxygen but it was not as deep as on the previous day, and we were able to cut down the oxygen supply to four litres per minute without causing the cyanosis to return. Administration was kept up continuously with intermissions of ten minutes every half an hour. The following day the oxygen could be safely cut down to three litres per minute, and it was given for half an hour at a time with intermissions of half an hour. The pulse was now 100 per minute in rate and of good quality; the respiration rate was forty, and the hyperpnœa much diminished. Later in the day it was possible to cut the oxygen down to two litres per minute. The following morning the oxygen was reduced to one litre per minute, and in the evening the administration was stopped altogether as the cyanosis did not return.

The second case had been gassed ten hours before I saw him by the gas shell which penetrated his billet. In the interval he had been treated intermittently with pure oxygen by means of a nitrous oxide inhaler. When I first saw him his condition was very grave and he appeared to be moribund. His pulse was hardly palpable and it was impossible to count the rate, and he exhibited a livid cyanosis. Five litres of oxygen per minute given by the Haldane apparatus caused a considerable improvement in colour, but even with ten litres per minute the colour did not seem to me to become quite normal. Continuous administration of oxygen was started with the rate of oxygen delivery of six litres per minute. The pulse became palpable in a few minutes, though it was of somewhat fluctuating quality; its rate was now 120 per minute. Half an hour after starting the oxygen the respiration rate was fifty-two and the breaths were not deep. Three hours later the pulse was 112 per minute in rate, and though weak it was quite regular; the respiration rate was fifty-eight. The patient objected to having the mask removed, and when it was taken off he soon made feeble efforts to regain it: I may add that during the period I am describing the mask of the apparatus was never removed for more than a minute or two at a time. Two hours later the pulse was 108 in rate and slightly stronger, the respiration rate being still fifty-eight. The patient still continued to resist the removal of the mask, and appeared to be able to sleep as long as he was having oxygen. At the end of another two hours the pulse and respiration rates were unchanged, but the

15

colour of the face whilst the oxygen was being administered was now very definitely better than it was during the first hour of oxygen administration. After a further two hours and a half the pulse rate had fallen to 100, the respiration rate being fifty-four. Five litres of oxygen per minute now caused a marked improvement in colour, and the administration was therefore continued with this rate of delivery. If the rate of oxygen supply was reduced to three litres per minute there was far less relief of the cyanosis. At the end of another hour and a half, i.e., about twelve hours from the commencement of the continuous oxygen administration, the pulse, though weak, was very definitely stronger than at the start, its rate being 102, the respiration rate had dropped to forty-eight per minute. The patient was more conscious and did not look so moribund while the oxygen was being given, but when the oxygen was stopped his condition deteriorated rapidly and cyanosis became extremely marked. The oxygen treatment was continued subsequently and the patient eventually made a good recovery. It was possible to transfer him to England on the tenth day and he rejoined his regiment in France within a year.

I have quoted these cases in detail because I think that they will bring home to you the reality of the dangerous effects of deficiency of oxygen on the central nervous system and on the heart. You will notice how quickly the relief of the cyanosis is followed by a great improvement in the quality of the pulse and by a reduction in the rate of the heart beat and the disappearance of irregularity in its action, how coma may give place to consciousness, and how the patient's unwillingness to be deprived of the oxygen may indicate the relief of the discomfort which he is experiencing. Stop the oxygen and the condition of the patient rapidly deteriorates. The improvement, striking as it is within a brief time of commencing the oxygen administration, becomes progressively greater as the administration is persisted with.

These instances will serve, too, to emphasize a point which I have already alluded to, namely, the necessity of administering the oxygen continuously in cases of this type where one is endeavouring to tide over an acute but transitory condition of asphyxia. I feel that, in the past at all events, medical men have not infrequently failed to recognize in their true light the serious effects that accompany any grave and continued shortage in the oxygen supply to the tissues of the body, effects that cannot be remedied by infrequent administrations of oxygen for but a few minutes at a time. I recall in this connexion that I have seen in France a patient suffering from acute pulmonary œdema, and deeply cyanosed, to whom oxygen was being administered for but five minutes every hour: yet the oxygen apparatus was standing by the bedside all the time in perfect working order, the cyanosis could be relieved immediately, and no great supply of oxygen was required to accomplish this, and the patient offered no resistance to the application of the mask since he found relief during the oxygen administration.

You will notice that in speaking of treatment with oxygen I have confined myself to the use of portable apparatus for its administration. Another method that may be employed is to place the patient in a closed chamber in which the air has been artificially enriched with oxygen. Such a method has obvious advantages. The patient is unhampered by any apparatus and the work of those attending him is facilitated. Against the advantages must, however, be set certain disadvantages. Such a chamber is costly to build and requires attention in the

running, while some system for preventing carbon dioxide from accumulating in the air must be adopted, and the risk of fire must be carefully guarded against. Evidently it would be difficult to establish an oxygen chamber anywhere but in a permanent hospital, and the number of cases that could be treated simultaneously would be limited. Chambers of this type were built in England during the war and used with excellent results,[1] and there is little doubt that the method will be developed further in the future for clinical use. A portable apparatus is, however, clearly essential in the field and can hardly be supplanted in ordinary medical practice owing to the limitations of the chamber method.

I have already drawn your attention to the venous engorgement and signs of right-sided dilatation of the heart which may be distinguished in the full blue cases of cyanosis, and the evidence of cardiac failure in the grey ashen cases will be clear from the descriptions I have given. Any failure of the circulation can but accentuate the anoxæmia determined by the pulmonary lesions and help to establish a vicious circle. There is yet another factor to take into account which I have not so far mentioned and that is a concentration of the blood with a rise in the hæmoglobin and red corpuscle content which occurs in all gas casualties suffering from severe pulmonary œdema, a concentration dependent, no doubt, on the abstraction from the body of the fluid exuded into the lungs, on the general condition of shock, and probably in some degree on the anoxæmia. An alteration of this character in the blood implies an increase in viscosity, and though such an increase may be tolerated with impunity in norma persons under certain circumstances, e.g., at high altitudes, where an increase in the number of red cells and in the hæmoglobin is one of the adaptive changes shown by the body to compensate for the reduced oxygen pressure in the atmosphere, it may well prove a serious factor in cases of gas poisoning in which the pulmonary capillaries have been damaged and very likely offer an increased resistance to the flow of blood. Should this effect develop you can well understand how the strain may prove too much for a heart already profoundly affected by the deficiency of oxygen.

In considering the effects of anoxæmia in these cases we must not, therefore, confine our attention solely to the primary cause of the shortage of oxygen which is to be found in the hindrance imposed on the passage of oxygen from the alveolar air into the blood; we must take into account the secondary cause dependent on circulatory failure in one or another form, and be prepared to adopt suitable treatment to counteract its effects. The importance of venesection was emphasized by Macaulay and by Irvine in cases of acute pulmonary œdema arising from accidental poisoning with nitrous fumes on the Rand,[2] and it was on the basis of this experience that venesection was first seriously practised in France when dealing with gas casualties resulting from cloud gas attacks. Of the value of venesection in the cases showing venous engorgement and full blue cyanosis there can be no doubt,[3] and the rapidity with which the abstraction of blood is followed by an amelioration of the patient's condition such as diminution of the cyanosis, and distinct reduction of the dyspnœa, headache and general discomfort, suggests that

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[1] Barcroft, Hunt and Dufton, *Quart. Journ. Med.*, vol. xiii, p. 179, 1920.

[2] Irvine, *Brit. Med. Journ.*, January 29, 1916.

[3] Hebblethwaite, *Brit. Med. Journ.*, July 22, 1916.

in the main the treatment owes its effect in these cases to an immediate relief of an embarrassed circulation at a critical period when the heart is beginning to fail and its right side to dilate under the strain. Numerous experiments, particularly by the American observers,[1] have established the beneficial effects of venesection on animals poisoned by the acute lung-irritant gases, but the point remains in doubt whether the abstraction of blood will hinder the exudation of fluid into the lungs or promote its re-absorption when once œdema has been established, and experiments made on animals by English observers have given but inconclusive results in this respect. Venesection of the grey, ashen cases that showed no venous engorgement was found to be of no benefit, and in fact sometimes appeared to do harm, and in this class of case the question of the possible benefit of infusion of saline must be considered. In the earlier days in France this treatment was not adopted, for the reason, I think, that fear was felt that the procedure might lead to a serious accentuation of the pulmonary œdema, though I believe that one or two grey cases were infused at a later period with apparently some benefit. The German official medical pamphlet issued during the war mentions that infusion had been tried in a few cases with beneficial results, but the treatment was not recommended enthusiastically. Towards the close of the war Colonel Underhill[2] and other American investigators brought forward a number of valuable results (*loc. cit.*) which they had obtained in a long series of experiments on animals which enabled them to make a strong case for treatment by infusion. They were of opinion that the concentration of the blood and the consequent alteration of viscosity was a real factor which contributed to a fatal issue, and that infusion would help to check the increase in viscosity. It would appear, therefore, that we overestimated the risk of infusion in these cases, and we might have been well advised to make a careful trial of the method in the grey cases, being guided by Underhill's suggestion that an index of the desirability of infusion could be obtained by following the hæmoglobin concentration of the blood. Apparently when once pulmonary œdema has been well established infusion of saline can be practised without accentuating the exudation. Venesection in itself will very likely lead to dilution of the blood owing to the tendency for liquid to be drawn in from the tissues to replace the volume of blood abstracted from the circulation. It should not be forgotten that though venesection may at first appear undesirable in any particular case, the improvement resulting from oxygen administration or perhaps from infusion of saline may so alter the picture as to suggest that venesection may prove of benefit at a somewhat later stage.

I have now taken up a great deal of time with a discussion of anoxæmia as it may occur after poisoning by the acute lung-irritant gases, and I have done so because I feel that we have here a particularly important type of case. We are faced by an acute shortage of oxygen which is determined primarily by the conditions in the lungs, and is secondarily accentuated by changes in the circulation which are largely at all events dependent on the primary anoxæmia. Immediate asphyxia is the real threat to life, and in comparison with this the part played

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[1] Report No. 13 of Chemical Warfare Medical Committee, published by the Medical Research Council, November, 1918.

[2] Underhill: "The Lethal War Gases: Physiology and Experimental Treatment," Yale University Press, 1920.

by secondary bacterial infection of the damaged tissues is as a rule of minor importance. It is true that secondary infection with consequent bronchopneumonia was of frequent occurrence in the experiments on dogs made by the American investigators, and they laid much stress on these sequelæ. In Dunn's experiments on goats, on the other hand, recovery almost invariably occurred without septic complication, which suggests that they more nearly represent the conditions which prevailed in man, for I have told you that in France our experience was that bronchopneumonia was an uncommon sequela.

The case is quite different when we turn to the effects produced by mustard gas (dichlorethyl-sulphide). This substance has a delayed but intense inflammatory action on the skin, the conjunctivæ and the mucous membrane of the respiratory passages. It may be discounted as a direct cause of acute pulmonary œdema, save perhaps in exceptionally severe cases, and it is only very rarely that the intensity of the primary inflammatory reaction may lead to so much blockage of the bronchial tubes by false membrane and exudation as to threaten death from asphyxia. A fatal result practically invariably depends on the secondary bacterial infection of the damaged and necrotic mucous membrane of the bronchial tubes and the development of severe bronchopneumonia with some degree of associated pulmonary œdema. As this condition becomes established cyanosis, which was absent in the earlier stages, may begin to make its appearance. In such a case there is, however, a further danger than simple anoxæmia, for the body has to contend with the bacterial intoxication. We may give oxygen to such a case and find it followed by relief of the cyanosis and by an improvement in the heart's action, and yet we may be unable to stave off a fatal issue. It is clearly right in such cases to persist with oxygen treatment, for if we can get such obvious signs of the relief of noxæmia, we can be certain that we are putting the body in a better condition for fighting the infection, though in the end the infection may get the upper hand. We may prolong life and give the patient a chance of pulling through in virtue of his own recuperative powers. If we can prevent the shortage of oxygen from developing it may just make the difference between life and death, though oxygen will not cure the pulmonary lesions, nor is it likely to influence the bacterial infection directly. When you recollect how severely the tissues of the body feel any shortage of oxygen you will see the wisdom of administering oxygen whenever you have reason to believe that cyanosis is dependent on the failure of the blood to get properly oxygenated in the lungs.

I do not, however, want to leave you under the impression that anoxæmia only arises from changes in the lungs which prohibit the free entry of oxygen into the blood. Leaving the question of the poisonous gases used for offensive purposes, let us for a moment turn our attention to the effects produced by two other poisonous gases, the occurrence of which may be regarded as due to accidental causes rather than to the deliberate action of man. The first of these is carbon monoxide. This gas is evolved in large quantities, as you are aware, on the explosion or detonation of explosives. It was met with, therefore, during mining operations owing to the explosion of hostile mines or camouflets, and was responsible for a considerable number of casualties. It penetrated into deep dugouts when a high explosive shell, with a delay action fuse, burst in the vicinity, and rendered the blow back from guns and machine-guns a source of danger.

Casualties were sometimes caused by this gas owing to its occurrence in the fumes arising from coke braziers or in the exhaust gases of motor vehicles. Carbon monoxide has in itself no action on the lungs; its poisonous action depends solely on the fact that it can combine with hæmoglobin and thus hinder the red corpuscles from fulfilling their proper function as transporters of oxygen, and were it not for this property it would be a purely inert gas so far as the body is concerned.

Carbon monoxide combines with hæmoglobin to form a dissociable compound just as does oxygen, but the affinity of carbon monoxide for hæmoglobin is roughly 240 times that of oxygen. When blood is exposed to an atmosphere containing both carbon monoxide and oxygen the proportions of carbon monoxide hæmoglobin and oxyhæmoglobin which are formed are determined by the laws of mass action, the hæmoglobin dividing itself between the two gases according to their relative partial pressures.[1] Thus if blood is saturated outside the body with normal air (20·9 per cent of oxygen), containing 0·085 per cent of carbon monoxide, the hæmoglobin will finally become equally divided between the two gases, or fifty per cent saturated with carbon monoxide. If the concentration of carbon monoxide in this air is raised to 0·255 per cent, equilibrium will be reached when the hæmoglobin is seventy-five per cent saturated with carbon monoxide; if it is reduced to 0·028 per cent, equilibrium will occur with the hæmoglobin twenty-five per cent saturated with carbon monoxide. A similar partition of the hæmoglobin in the blood between carbon monoxide and oxygen occurs when a man breathes air containing a small proportion of carbon monoxide, and as the oxygen carrying power of the blood is progressively diminished symptoms of anoxæmia begin to show themselves. Yet reduction of the oxygen carrying power of the blood by saturation of a certain proportion of the hæmoglobin with carbon monoxide has a far more severe effect than reduction of the hæmoglobin to a similar degree by some form of anæmia, for with partial saturation of the hæmoglobin with carbon monoxide the dissociation of any oxygen carried by the hæmoglobin is rendered more difficult, the oxygen being in fact liberated in the tissues at far lower partial pressures than would otherwise be the case.

With massive doses loss of consciousness may be rapid, and death may be preceded by convulsions. With more moderate doses the symptoms develop gradually since the gas is only absorbed slowly, and present a typical picture of the effects of progressive and uncomplicated anoxæmia. Loss in power of the limbs, giddiness, confusion of mind, and breathlessness and palpitation on the least exertion are succeeded by apathy and complete helplessness, the failure in the intellectual powers gradually passing into complete unconsciousness which may finally terminate in a painless death. The extent to which the symptoms develop is dependent on the degree of saturation of the hæmoglobin with carbon monoxide, which is eventually reached under the mass influence of the carbon monoxide and the oxygen in the air breathed, and on the length of exposure when once equilibrium is attained.

Here again a recognition of the way in which carbon monoxide exerts its effects on the body suggests the proper lines of treatment. As carbon monoxide is a

[1] Douglas, J. S. Haldane and J. B. S. Haldane, *Journ. Physiol.*, vol. xliv, p. 275, 1912.

dissociable compound, the unfettered mass influence of the oxygen in the air begins to expel the gas from its combination with hæmoglobin as soon as an atmosphere free from carbon monoxide is reached, This process is slow owing to the high affinity of carbon monoxide for hæmoglobin, but in an hour or two the blood will be entirely freed from the gas. The normal alveolar air in the lungs only contains about fourteen per cent of oxygen when ordinary air is breathed, so that the administration of pure oxygen will render the expulsion of carbon monoxide six times as fast. The right procedure is therefore to administer pure oxygen continuously to the patient (taking precautions that the expired air is not rebreathed), when we may expect the whole of the carbon monoxide to be expelled from the hæmoglobin, and the normal oxygen carrying power of the blood to be restored after half an hour to an hour. Artificial respiration may sometimes be necessary at first if the breathing threatens to fail, and in any case it is obvious that rest is essential. You should note that in this case we use pure oxygen for a short time with the deliberate object of hastening the expulsion of carbon monoxide from the blood, and that when once the oxygen carrying power of the blood has been rendered normal there is no value in continuing the oxygen administration. The purpose for which the oxygen administration is required is quite different from that which we discussed when dealing with acute pulmonary œdema. In spite of the fact that all the carbon monoxide has been got rid of from the blood the patient may not recover consciousness for a number of hours, and he may even die without regaining consciousness. This gives you some idea of the profound damage which may have resulted in the body during the period of severe anoxæmia, and evidence of a less degree of damage is not infrequently seen in cases that recover in the shape of partial paralysis, mental disturbance or cardiac dilatation.

I want to contrast the grave symptoms of anoxæmia that are seen in carbon monoxide poisoning with the effects produced by another poisonous gas which causes a diminution in the oxygen carrying power of the blood. I refer to the action of arseniuretted hydrogen. This gas is apparently strictly cumulative in its action, the effects produced being practically proportional to the product of the concentration of the gas and the duration of exposure. Thus exposure of animals for about six minutes to one part of arseniuretted hydrogen in 1,000 of air causes death to occur about twenty-four hours later, and the same result ensues after an exposure for twelve hours to a concentration of one part of the gas in 100,000 of air. The gas acts upon the red corpuscles of the blood causing intense hæmolysis, and this effect seems to be at the bottom of the symptoms. In slight cases there is only jaundice and anæmia, associated with a feeling of malaise, nausea and pains about the body. In more severe cases there is hæmoglobinuria as well as jaundice, and bad secondary anæmia. In bad cases vomiting sets in in an hour or two with intense pain in the back, weakness, fainting and collapse, hæmoglobinuria is intense and acute nephritis occurs with casts in the urine. Death seems to be due either to the nephritis, or to the want of oxygen caused by the destruction of the red corpuscles and loss of hæmoglobin. The nephritis appears to be largely dependent on the accumulation of hæmoglobin or its derivatives in the kidney. Peripheral neuritis is a not uncommon symptom in the later stages of non-fatal cases.

We never encountered this gas during the warfare on land, but a certain

number of cases occurred amongst the crews of submarines, since in certain instances small amounts of arseniuretted hydrogen were formed during the charging of the accumulators, the lead plates of which at times contained traces of arsenic. A good description of these cases has been recently given by Surgeon Lieutenant-Commander Dudley, R.N.[1] The symptoms were moderately severe in some of the cases, but fortunately no fatality occurred. Some of the cases admitted to hospital showed only two million red corpuscles per cubic millimetre with a hæmoglobin percentage reduced to half the normal amount. Yet in spite of this symptoms of actual shortage of oxygen were extremely slight. Unusual breathlessness on exertion was probably to be attributed to this cause, and perhaps the headache which was a constant and troublesome feature should also be ascribed to some shortage of oxygen.

A man whose hæmoglobin is fifty per cent saturated with carbon monoxide is in a helpless condition—his mental power is profoundly affected, he staggers like a drunken man if he attempts to walk and probably collapses with any exertion—yet a reduction of the hæmoglobin by fifty per cent in a case of arseniuretted hydrogen poisoning gives no such evidence of grave deficiency of oxygen. I have already given you a clue to the explanation of these differences. The presence of carbon monoxide profoundly modifies the dissociation of oxygen from the hæmoglobin, oxygen being yielded up by the hæmoglobin with far greater difficulty than usual as the blood passes through the tissues. There is, however, no such influence when the hæmoglobin has been merely reduced in amount in the blood in consequence of the hæmolysis caused by the arseniuretted hydrogen. The hæmoglobin that is left will pick up oxygen in the lungs and yield it up in the tissues just as it does under normal conditions, and the reduction in the total oxygen carrying power of the blood can be counteracted by an increase in the circulation rate. If, however, the oxygen requirements of the tissues when the body is at rest demand that the blood shall be circulated at a rate considerably above the normal, it is clear that the power to meet any increase in the oxygen requirements must be greatly limited. Hence one finds that muscular exertion demands more of the circulation than it can accomplish, and the failure of compensation is shown by the unusual degree of breathlessness which hinders the subject from doing more than a very moderate degree of muscular work. We should find an analogous state of affairs in any other condition of severe anæmia.

Finally let me quote an instance of anoxæmia which occurred during the war under circumstances quite different from any that I have hitherto mentioned. A grain ship which had been sunk at sea was eventually salved and towed into harbour. Some time later a party of men was set to work to clear the hold. At first all went well, though on one or two occasions the men noticed a curious dizziness and weakness of the limbs which passed off quickly if they stopped work for a few moments. One day, however, the working party immediately after descending into the hold was overcome without previous warning by dizziness, loss of power of the arms and legs, and loss of consciousness. It was possible to drag some of the men rapidly from the hold when they quickly recovered, and no symptoms developed later on, though some had headache for a

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[1] *Journal of the Royal Naval Medical Service*, vol. v, p. 239, 1919.

time, but three men died before they could be rescued. It was possible to exclude carbon monoxide definitely on this occasion, nor was there anything pointing to the fact that the men had been overcome by such a gas as sulphuretted hydrogen which might have been generated in the decomposing cargo. There is little doubt that what really happened was that the men entered unknowingly an atmosphere grossly deficient in oxygen owing to the absorption of oxygen from the air in the hold by the sodden grain.[1] The fact that none of them had noticed any marked breathlessness on entering the hold on the previous days suggests that the reduction of oxygen in the atmosphere was not accompanied on this occasion by any material accumulation of carbon dioxide, though the slight symptoms that they did experience pointed to something being wrong with the air.

I have tried to-day to give you some idea of the different conditions under which we had to face symptoms due to anoxæmia during the war as a result of gas poisoning, and to show you that we have got to reckon with different causes for this anoxæmia. I have dealt with the treatment of these cases only in so far as it is founded on a knowledge of the factors at work and itself helps to explain and emphasize these factors: I had no intention of entering fully into the question of treatment. I want you to appreciate the relative importance of anoxæmia in the different types of casualties, and to see that though under some circumstances acute anoxæmia may constitute the gravest danger that threatens the patient, at other times it forms but a contributory factor in the course of an illness in which perhaps the danger arising from secondary infection or more directly from some further toxic property of the gas occupies the most prominent position. Yet I hope you will agree with me that even in the latter case the alleviation of the prejudicial effects of deficiency of oxygen on the cells of the body may just turn the balance and make the difference between life and death.

Current Literature.

Pneumonia.—Review from *Medical Science Abstracts and Reviews*, September, 1920, of the Medical Research Council. Zander describes an extensive outbreak of severe pneumonia due to Friedländer's bacillus which occurred in a prison camp between December, 1916 and April, 1917. There were 411 cases with 144 deaths, a mortality of 36 per cent.

Bacteriology. — Spooner has collected the results of the bacteriological examination in 2,980 cases of lobar pneumonia, 1,103 being under his own observation at a U.S.A. base hospital. Twenty-nine per cent were caused by type 1 pneumoccoccus, 21 per cent by type 2, 11 per cent by type 3, 39 per cent by type 4.

A new method of typing pneumococcus is described by Hirshfeld, Loewe, and Wallach, based on the observations of Rosenow and also of Longcope, that a coagulum is formed if pneumococci are grown in the blood of a pneumonia patient (*J. Am. M.*, Aug., 1919, 73, 170-1). This clotting is thought to be due to the production of acids probably from the glycoproteins of the blood serum. This

[1] Frederick (*Journal of Hygiene*. vol. xix. p. 205. 1920), gives data which shows how rapidly the oxygen may be used up when vegetable matter is stored in ill-ventilated spaces.

ability to form a clot is found by Hirshfeld, Loewe and Wallach to be specific for the different types of pneumococcus. Briefly the method is as follows: A small quantity of the patient's blood is laked with ether, the red cells are removed and the fluid added to tubes containing a saline emulsion of types 1, 2 and 3 pneumococcus, a fourth tube being used as a control. The emulsions must be made from actively growing cultures and sterile precautions observed. The tubes are kept in the water bath at 37° C. until the colour changes appear. The chemistry of the reaction is not known exactly, but hæmoglobin derivatives first appear, giving a brownish red colour, finally a dark brownish red gelatinous clot is formed. The average time before the colour change takes place was six to eight hours, the shortest two hours. The authors suggest that possibly spectroscopic examination might help to an earlier reading. Tests were done in fifty-two cases, and in forty-nine the results were confirmed by agglutination with organisms isolated from the sputum or after puncture of the lung. In the other three cases no sputum could be obtained as it was twelve to fourteen days after the crisis.

The test was found to be positive as early as the second day.

No opportunity was offered of taking blood on the first day.

In three cases the reaction was absent after crisis.

Two advantages are claimed for the reaction: (1) Blood is easy to get; (2) Mixed infections can be determined.

The fact that a very extensive trial has not yet been given is the unfortunate drawback.

Symptomatology.—Zander describes some of the clinical aspects of a severe form of endemic pneumonia due to Friedländer's bacillus. The main points are: (1) There was usually a prodromal stage of one or two days marked by general malaise. Only one-third of the cases began with a sudden chill; (2) In by far the largest number of cases the disease attacked the lower lobes; (3) On an average fever persisted 9·5 days, the extremes being two and forty-eight days; (4) Recovery by crisis occurred in 131 cases, by lysis in 127.

At the beginning of the epidemic the cases were like true croupous pneumonia, but later on more like broncho-pneumonia.

It is of interest to note that after the patients were convalescent their sputum even for a very long time contained Friedländer's bacillus.

Sofre thinks that central pneumonia does not receive the attention it deserves, the general impression that physical signs are absent he disputes. It is true that dulness and blowing breathing may not be found either early or late in the disease; if these signs appear about the fifth day the author terms the disease centrifugal pneumonia in contrast with true central pneumonia where the inflammatory process never reaches the surface of the lobe. In central pneumonia dyspnœa, rusty sputum, and pain in the side may not be present, but it is not true that the cough is lacking; the physician must question most carefully on this point. Sofre has investigated these cases most carefully and has recorded two findings of great aid in diagnosis: (1) Over one lobe—perhaps over the whole of it—there is a hyperresonant note as compared with that of the opposite side; (2) The breath sounds are relatively feeble. The author explains these signs by assuming that there is a peripheral emphysema about the centrally placed consolidated lung.

Robitschek discusses the difficulties in explaining the vagaries and apparent inconsistencies in the physical signs of lobar pneumonia, and especially in those cases which simulate pleurisy with effusion. There is dulness, but breath sounds are absent, voice sounds are decreased, and tactile fremitus is impaired. The explanation may be that the bronchi are blocked with fluid or mucus, or there may be a massive pneumonia where the bronchi are involved in the general inflammation and are filled with fibrous exudate. Other cases have been seen, however, at autopsy, where neither of these explanations will hold. The author suggests that in certain patients with a rigid or paralysed thorax, when the lung

becomes large or hepatized, the thoracic wall does not give, and the ribs make deep impressions on the surface of the organ and at the same time, by the increased pressure in the pleural cavity the bronchi become flattened out. Thus breath sounds and voice are poorly transmitted. At autopsy after the thorax has been opened it is impossible to prove this point.

Lissner makes a plea for the advantages of employing the whispered voice in defining areas of consolidation, especially in influenzal pneumonia.

Complications.—Stone describes 72 instances of pericarditis in 300 post-mortems on pneumonia cases in a base hospital at Fort Riley, Kansas. 14 of the cases were of the sero-fibrinous variety: 11 of these were in lobar pneumonia and 3 in bilateral broncho-pneumonia. Acute purulent pericarditis occurred in 44 cases: in 30 of lobar pneumonia, in 11 of broncho-pneumonia, and in 3 of the cases both types of pneumonia were present. Subacute fibrinoplastic and purulent pericarditis (shaggy heart) were observed 14 times with lobar pneumonia and with broncho-pneumonia in 6 cases. In about 75 per cent of cases the pericarditis was caused by the streptococcus, usually of the hæmolytic variety. The organism generally corresponded with that isolated from the pleural fluid. In 33 cases there was bilateral pneumonia, in 18 pneumonia of the right lung, and in 21 cases pneumonia of the left lung. The most important factor in the development of the pericarditis was empyema, as 63 out of the 72 cases had this complication as well.

The most important sign in the diagnosis of pericarditis was a to-and-fro friction which sometimes could be felt, and occasionally this was best heard below the angle of the left scapula, being transmitted through consolidated lung, although generally made out at the base of the heart. X-ray was most useful in diagnosing effusion. A positive venous pulsation in the neck was sometimes present if the pericardial sac was greatly distended. Pericarditis was suspected in those patients who continued to have a rapid pulse and a septic temperature after aspiration or operation for empyema.

In Stone's opinion the condition of the patients never warranted incision and drainage of the pericardial sac. In tapping the pericardium the needle was inserted outside the nipple-line in the fourth interspace and pointed inwards and backwards towards the spine. Or again it was inserted beneath the costal margin in the narrow angle formed by this and the xiphoid cartilage, but often with empyema present the diaphragm was higher on the affected side than normal, and this spot was unavailable. The needle should be kept close behind the costal margin and pointed towards the left nipple.

In the discussion of Stone's paper one of the speakers said there was less danger in puncturing the heart-wall than was generally thought, as he had often done this without causing any harm. He also stated that a small but rapidly increasing effusion, where the sac has no time to stretch, may kill quickly, where a more slowly forming effusion may be well borne. Another speaker had found pallor of the lips, probably due to distension of the sac with fluid, an indication for tapping the pericardium immediately.

Roussel and de Lavergne discuss the significance of five examples of jaundice which occurred during the third wave of the epidemic of influenza. Two of the patients showed signs of lobar pneumonia of the left lobe and the others broncho-pneumonia. Blood culture for the pneumococcus was positive in all cases. Bile pigments were found in the urine of all, only some showed clay-coloured stools. There was enlargement of the liver with tenderness. Nephritis was present. One patient developed signs of meningitis three days after the jaundice; the cerebrospinal fluid was purulent and contained many pneumococci. Four of the patients died and the fifth received 100 cubic centimetres of anti-pneumococcus serum daily. The authors believe that the bile retention was due to a lesion of the liver cells caused by the pneumococcus. In two cases it was clearly shown that it was a dissociated retention of bile, and not retention of the bile as a whole, as there were no bile salts in the urine, no itching of the skin, no slowing of the

pulse, and in one case examined no evidence ot bile salts in the blood serum. The bacteriolytic action of bile and its inhibitory action on the growth of the pneumococcus is well known. The writers prefer to explain the positive blood cultures in their five cases by the fact that there was no retention of bile salts, rather than by the supposition that the pneumccoccus was protected by the albumin of the blood or tissues.

Flandin, Debray and Francon state that a blood infection with the pneumococcus, with cr without localizing symptoms in the lungs, may be accompanied by signs and symptoms of acute peritonitis. These cases may recover without operation. On the other hand the septicæmia may become more severe and the patient die rapidly, and show at the autopsy only a very congested peritoneum with only a few cubic centimetres of turbid exudate and a very distended intestine. In the third and most common form, suppuration takes place and diffuse general peritonitis results, which rapidly causes death if no operation is undertaken.

Treatment.—Tenney and Rivenburgh give their experience in the serum treatment of sixty-eight cases of lobar pneumonia due to type 1 pneumococcus. All the cases except four were in coloured soldiers. On admission if lobar pneumonia was diagnosed or suspected, ½ cubic centimetre of horse serum was given subcutaneously to desensitize the patients. At the same time sputum was sent to the laboratory to be typed. From four to six hours after admission the first dose of serum was given. This caused slight if any change in the blood picture. Slight anaphylaxis occurred three times. A thermal reaction, comparable with that which so often follows the intravenous injection of any foreign protein, showed itself in only a few instances. This reaction was always favourable and several times resulted in prompt crisis. Serum sickness coming on from four to sixteen days after the last injection was practically seen in all the cases; the urticaria or asthma were relieved by a hypodermic injection of 0·5—1 cubic centimetre of epinephrin in a 1 in 1,000 solution.

The serum was administered in 100 cubic centimetre doses and given at body temperature every eight hours night and day until the rectal temperature registered 100° or lower. Sixty-one of the sixty-nine cases received from 30 to 900 cubic centimetres of serum with an average of 344 cubic centimetres. Of the seven who received no serum six were already convalescent when the sputum was typed and the sputum of the remaining one showed type 4 pneumococcus and *B. influenzæ*; at the autopsy on this last case type 1 alone was recovered from the heart's blood.

Thirty of the 61 patients recovered by crisis, 22 by lysis and 9 died. In addition to the serum treatment, abundant fresh air was afforded, and digitalis and whisky were given in the critical stages, and codein was used as a sedative if necessary. Workers in the Rockefeller Institute place the mortality in type 1 pneumonia, who do not get antiserum (type 1), at twenty-five per cent, and in a series of 101 cases treated with serum the mortality was only 7·5 per cent. The mortality of the series of Tenney and Rivenburgh was 14·7 per cent. This higher rate may be due partly to the lower resistance of the coloured people, to the season of the year, and to the forced moving of the patients at the beginning or early in the course of their illness.

Cohen treated all his cases with quinine. He uses the dihydrobromide by mouth, quinine and urea hydrochloride for intramuscular injection, and for intravenous therapy either of these or the dihydrochloride. The drug is not given to reduce the temperature but the degree to which this is lowered is taken as an index to the extent of the pharmacological action of quinine. Cohen states that the bactericidal properties of the blood of cinchonized animals and men, and the power to prevent growth of cultures of pneumococcus in vitro, are retained much longer if the quinine is given in frequent doses than if given in larger ones at greater intervals. Cohen keeps the temperature down to 100° F. or less; by mouth twenty-five to thirty-five grains are given at once, and later five to fifteen

grains every two, three or four hours to bring about the desired effect. Quinine he says is very well tolerated by pneumonia patients. Cohen highly recommends the use of pituitary extract (posterior lobe) in subcutaneous doses of one cubic centimetre every third hour until the diastolic pressure in millimetres exceeds the number of heart beats by five points per minute. He emphasizes the virtues of this and also of eserin in the treatment of tympanites. He employs digitalis to sustain the diastolic blood-pressure.

Sterling makes a plea for the early use of atropin in pneumonia. He emphasizes that it should be given early, and points out that in influenza a very frequent finding at autopsy is a wet lung. He advocates the use of atropin in influenza even before signs of pneumonia are present. Sterling gives it hypodermically in doses of 1/150 to 1/100 of a grain or five to ten drops of the tincture of belladonna. He lost only eight cases out of 120 of clinically definite pneumonia, and two of these were moribund at the first visit. His success, it appears, might as easily be attributed to pituitrin and to digitalis which were used all through the illness.

Weaver gives sodium citrate in pneumonia and employed this treatment in forty-seven cases, some of which were very severe. In forty-five of these the duration of disease was decreased and a quick defervescence by crisis or by rapid lysis was brought about. This effect was produced in forty-eight hours up to four days. According to Weaver the *sine qua non* for a cure in pneumonia is the re-establishment of the cardio-pulmonary circulation in the hepatized lung, and this sodium citrate helps to do by decreasing the coagulability of the blood which is always increased in pneumonia. Sodium citrate also helps to maintain the alkalinity of the blood. It is given to adults every hour day and night with an abundance of water in doses of from 0·9 to 1·2 grammes or 2·4 grammes every two hours. Should diarrhœa result a mild opiate is given. This form of treatment is claimed to be equally efficacious in frank lobar pneumonia and also in cases of influenzal broncho-pneumonia.

Review.

DISEASES OF THE THROAT, NOSE AND EAR. By Dan McKenzie, M.D., F.R.C.S.E. London : W. Heinemann, 1920. Pp. xv + 646. Price 42s. net.

A work on his speciality by the Editor of the *Journal of Laryngology, Rhinology and Otology* must necessarily arouse interest. Dr. McKenzie claims that his book is written from a practical point of view, and that considerable attention has been devoted to operative surgery. In so large a subject much has been omitted from considerations of space, but on the whole the arts of selection and compression have been exercised with judgment, and the result is a work which is a fairly complete presentation of the subject matter. The largest part of the letterpress is devoted to the ear and we notice especially an excellent description of Bárány's tests for internal ear disease. Anatomical details have not been enlarged upon, but chapter xvi contains a good description of the lymphatics of the head and neck. The illustrations, many of which are original, are clear and well reproduced. A few typographical errors will no doubt be corrected in subsequent editions.

E. M. P.

Correspondence.

PARASITES (? DEVELOPMENTAL STAGES OF *SPIROCHÆTA RECURRENTIS*) IN THE LIVER OF A FATAL CASE OF MESOPOTAMIA RELAPSING FEVER.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—With reference to the above paper which appeared in the Journal of November, 1920, I should be grateful if you could find room for the following comment on that paper which I made at the meeting of the Royal Society of Tropical Medicine on February 18, 1921, when delivering a paper on "Some Observations on the Pathology of Relapsing Fever."

Referring to the "bodies" found in the liver I said: "I was unable to determine the nature of these bodies and came to the conclusion that they could not be leucocytes or degenerating nuclei, and thereupon, perhaps somewhat hastily, sent a note to the JOURNAL OF THE ROYAL ARMY MEDICAL CORPS suggesting that they were protozoal bodies, possibly having some relation to the spirochæte, and waited expectantly for more post-mortem material.

However, I have not been able up to the present to extend this research, and recently returned to a careful reconsideration of the original material, with the result that my faith in the accuracy of my former deduction has been shaken. In this state of mind I showed specimens to Dr. Wenyon and Dr. Stevenson, whose criticisms, though kindly, did not help to dispel my doubts.

If these bodies are not of a specific nature, then they are probably a phase of degeneration of polynuclears, but there is to me something unique in the characters of the nuclei when well stained that I find difficult to attribute to degeneration."

The characters of the bodies as depicted in my first paper are probably to be explained by some alteration in the staining reaction of the nuclei of the leucocytes, whereby only the centre of the nucleus takes up the stain, leaving a clear marginal zone and sometimes a central spot. This explanation will be readily understood by reference to the following rough diagram.

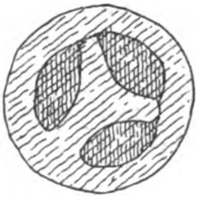

FIG. 1.

FIG. 2.

FIG. 1.—An unaltered leucocyte with normally stained nuclei.
FIG. 2.—An altered leucocyte in which only the central part of the nuclei has taken up the stain.

It would seem worth while to draw attention to this point if only to emphasize the difficulties that beset the research worker.

I am, etc.,
J. C. KENNEDY,
Lieut.-Col. Royal Army Medical Corps.

Royal Army Medical College,
June 3, 1921.

PHYSICAL TRAINING IN THE ARMY.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—Lieutenant-Colonel Cotton's letter on "Continued Physical Training of Troops" (after their period of recruit training in depots), opens up a subject of considerable importance.

There is little doubt that the average trained soldier in barracks is not so fi as the majority of six-months' recruits, and the average reservist still less so. In other words, the trained physique of the six months' recruit tends to som extent to deteriorate, by reason of the fact that games offer a means of maintaining tonus which are not always compulsory, and which are the hobby of the few rather than the necessity of all. The tendency to play the part of onlooker rather than that of performer has to be overcome. How effective the practice of universal games proved in the war will be recalled by many who interested themselves in such matters; and the Japanese practice of regimental running games commended itself to the British observer officers in 1904 as one of the greatest value. The effects of physical training and games on the recruit are obvious. Dettling ("Le Corps Humain") showed that the average French recruit improved very greatly in agility after his training (as tested by timing him over a quarter of a mile). It would be interesting to have data as to how far men relapse from such standards after ceasing their recruit training.

It is in manœuvres and war, however, that the unevenness of physical fitness between individuals shows itself most clearly. There is almost always a phase of heavy march casualties ere yet the less fit are hardened to the rigours of campaigning. The retreat from Mons showed that even our Army of 1914 suffered in like manner. Foot conditions and other ancillary factors being admitted, it would still appear economical to have *all* the troops on the same physical level; and the maintenance of physical training throughout a man's service would do much towards this ideal.

Marching, *under the load and conditions of war*, would appear perhaps to be the most valuable training of all. Luard, in pre-Crimean days, advocated occasional marches of formed bodies of troops from, say, Aldershot to Edinburgh; and the suggestion is eminently sound.

We may note that the Roman Republican armies, whose physical condition earned the admiration, not only of contemporary observers, but of all later critics, placed particular stress on the training of their recruits: a full description of this training—and a testing one it was—is given by Vegetius ("Epit. rei mil.," A.D. 395). But the training of the soldier was continued after embodiment in the Legions, and for so long as his service continued. At least three times in each month the legionaries were required to undertake a twenty-mile march in full equipment at the normal rate (militare gradu) with intervals of quick time (pleno gradu), and over country of every degree of irregularity. Under the Emperors the custom declined, and as a result the troops, not inured to marching under the load of war, were at first disinclined, and later unable, to carry the old equipment and to perform marches comparable to those of the Republican armies. To such an extent did this disorganization proceed that, despite Vegetius' plea for the re-establishment of the ancient practice of training, the Roman armies gradually succumbed and disappeared before the hardy barbarians of the North.

Many arguments might be supplied in favour of Lieutenant-Colonel Cotton's plea, and it is to be hoped the subject will be further ventilated.

I am, etc.,

N. V. LOTHIAN, *Major R.A.M.C.*
Assistant Instructor, Royal Army Medical School of Hygiene.

Blackpool,
June 14, 1921.

Notices.

EDITORIAL NOTICES.

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Journal
of the
Royal Army Medical Corps.

Original Communications.

THE LOAD CARRIED BY THE SOLDIER.

BY MAJOR N. V. LOTHIAN, M.C.
Royal Army Medical Corps.
Army School of Hygiene.

I.—GENERAL.

STRATEGY is the science of war, and, like all sciences based on immutable principles, demands precision in the working out of its conceptions. Inevitably therefore the logistics of war, defined by Colley as "the scientific combination of marches, the calculation of time and distance, and of economy of men's powers," stand out as perhaps the most important subject of study for the military student anxious to appreciate the lessons of past wars. And yet there is less exact literature on this very matter than on any other branch of military history. The question of "the economy of men's powers" is so vital that an inquiry into the question of how it can best be assured is of the most pressing nature. In this memorandum an attempt has been made briefly to consider the question of the load which has actually been carried by the organized soldiery of different nations through the ages, so that we may judge and assess the question in the light of actual experience. The question of the load carried cannot of course be considered entirely apart from other closely associated questions; and while peace-time conditions are of some value (and peace training is essential), the modifying factors inevitable under war conditions make it necessary to consider the question from all angles, and to allow for the interaction of the various factors involved.

In war, marching is unquestionably the military operation of first importance, involving as it does the transference of troops to the point

where the Commander decides to strike with maximum effect; in other words, while fighting is the luxury of the soldier, marching is his daily bread. But if the stroke is to be delivered effectively, the troops must still—after the march—be in condition to continue great physical exertion, and perhaps to repeat the march for an indefinite period. In other words, the march must not involve complete exhaustion, and therefore justifies close scrutiny of all those factors which make for the production of fatigue. Of these, the principal are the length of march, the time taken to cover it (i.e., the rate, and the halts), the load carried, and the men's physical condition, the latter factor in itself complicated by questions of dietary, clothing, climate, disease, and "the psycho-physical state of morale." It has appeared therefore most convenient for the purpose of the present inquiry to scrutinize primarily the question of load, noting however those other factors which necessarily enter into consideration, and examining the effect of reduced load as in itself favourably affecting the mobility and combatant value of troops. It will be seen, I think, that history bears out the thesis that, *ceteris paribus*, reduction of the soldier's load favours the troops whose powers are so husbanded, when meeting others normally bearing a heavier weight. It is essential to qualify by the words "*ceteris paribus*" inasmuch as load is only one factor involved; and obviously the physique or build of the troops dominates the question of load—matter worthy of notice where data are obtainable. By reason of the scanty references to the "Q" branch of wars in the past, a very moderate amount of exact detail only is available, but perhaps the following outlines may throw some light on this, at present, dark subject.

II.—The Effect of Load on the Rate of March.

The speed of the march bears an intimate relation to the load, and to the effects on the men's fitness. Analysis of speed converts it into the two component factors of length of pace, and rate (usually given as the number of paces per minute). As to the length of the pace that must necessarily diminish with a heavy load. It is usually accepted that the natural pace is $\frac{6}{7}$ of the height of the limb, and this averages some twenty-seven inches. Lemoine (*Tr. d'Hyg. Mil.*) states that the French average pace works out at twenty-five inches. Nevertheless we find the regulation pace of the British Army is 30 inches, and of the French 29½ inches (pas-de-route works down to 26 inches—Parkes). The Russian pace is 27½ inches, and the German thirty-one inches (*Regs. de l'Infanterie*, 1905). With a 60-pound load, Parkes decided 30 inches was quite long enough, if not actually too long, and showed that the pace usually shortened after a march of about 6 miles: Jackson independently advocated 27 inches as the regulation pace.

As regards the rate, Marey showed that with rates up to 130 paces per minute, there was no appreciable alteration in the length of each pace, i.e., the faster the rate, the quicker proportionately would any set distance

be covered : but that a rate of over 130 to the minute involved a reduction in the length of each pace. He recommended, for a loaded man, a rate of 105 to 125 paces per minute. The standard British and German rates are both 112 to the minute, and the French 120 (pas-de-route 100, rising to 110); these data as to pace and rate are however to some extent academic and apply to training and formal drill more than to the march, where indeed the men usually fall into their own pace and rate. The resultant optimum speed of loaded troops is usually estimated at about 2½ miles per hour, allowing for the very necessary hourly halt, but may be less with long columns. Stonewall Jackson laid down two miles per hour and a ten minutes halt. The French, according to Lemoine, average about 2·7 miles to the hour including the halt. It appears that for troops of the Western nations, loaded with kits averaging fifty to sixty pounds 2½ to 2¾ miles per hour is the optimum speed, and this has received a measure of scientific corroboration. It has been found that men carrying such a load march at the least cost to themselves at a rate comparable in most cases to the above; and that if the pace is slowed down (as in checks) it actually involves a higher energy output, just as does any acceleration (*vide* the paper by Cathcart, Lothian and Greenwood, *Royal Army Medical Corps Journal*, 1920). The practice of allowing men to regulate their own pace and speed, when they will march most economically, is thus apparently justified and such conservation of energy on the march will often balance the lack of military precision which results. When the kits are excessively heavy, or the ground unusually difficult, the rate is reduced accordingly. One finds that Napoleon's troops commonly took eight hours to struggle along as many miles in Poland. The Prussians moved from Wavre to Waterloo at a rate of some 1½ miles per hour only. The Austrians at Sadowa took twelve hours to cover fourteen miles. While so recently as the Russo-Japanese War our "Officers' Joint Report" informs us that one mile per hour was all the Russians could calculate on in moving their troops. Such figures indicate the effect of load on *rate* of march.

III.—Effect of Load on the Length of March.

As regards the *length* of the march, one must distinguish between normal marches, and those occasional marches when every mile counts, and troops are kept going near to the limit of exhaustion by reason of the urgency of some great crisis. Dealing with normal marching, the "distance covered before the onset of exhaustion is inversely proportionate to the load carried," and "all long marches of our own and other armies have been made by men carrying only small loads such as arms and a portion of ammunition" (Notter and Firth, *Hyg.*). Generally speaking, with the average load of fifty to sixty pounds with which the infantry of most modern armies are cumbered, about twelve miles in the day (of say eight hours' march, including long halt) appears to be the limit beyond which

exhaustion supervenes; if we reckon on a halt every fourth or fifth day, the daily average reduces to about ten miles. This is below the estimate of some writers (e.g., Schellendorf postulates fourteen miles), but agrees with Colley's and with Rustow's (*L'Art de Guerre*), and, more important still than opinions, agrees with the findings of history. Careful calculations show Xenophon's march of the Ten Thousand to have averaged about this figure, although somewhat higher figures are often claimed on insufficient evidence, *vide* the lecture by Sir G. Wolseley, *Journal Rl. U.S.I.*, 1873, and a study of European campaigns bears out the same truth—(see the excellent chapter in Furse's *Art of Marching*). Even the iron troops of Cromwell only averaged the same (*Anglia Rediviva*). Marches of this length (occupying, at 2½ miles per hour, and with an hour's midday halt, some six hours daily), can be sustained over considerable periods, provided that in the second or third week a mid-week rest day is allowed. When longer distances are covered the strain proves too great for continued effort, and involves straggling and exhaustion. Here the influence of load is marked and the lesson obvious, that if longer marches are to be obtained, with troops who will still be fit for fighting, the load must be reduced. When this is done the marching power of troops increases markedly: it is clear that a man of a certain potential energy will have more of that energy available for marching according as the amount he has to spend on carrying dead weight is reduced. And history demonstrates clearly how effective such reduction proves. De Chaumont held that while in India a man could do twenty miles without load with fair ease, twenty miles loaded is too hard a march to repeat for any length of time. The famous march to Candahar was made possible by reducing the load to thirty-four pounds (Crawford, *A.M.D. Report*, 1880). Osman Pasha's march to Plevna will be referred to later; it averaged over fourteen miles a day and was made possible only by the light load carried by his men. Jackson's Shenandoah Valley march (q.v. later), covered 670 miles in forty-eight days; while many other examples will rise to mind. The converse is equally true: by increasing the load the length of march is reduced at an increasingly marked rate, as witness our own troops in the late war. In the Russo-Japanese war, General Stakelberg, "who was given to getting the most out of his men," found that he could not allow a greater average day's march than four miles, his men bearing a weight of sixty-eight pounds kit plus addenda, the total sometimes approximating to eighty pounds (*British Officers' Joint Reports*). The moral is apparent, and need be stressed no further, but detailed examples of the effect of load will be given in the more purely historical sections following.

IV.—The Mode of Carriage of the Load.

The way in which the load is carried on the man's body would appear to be the next point to discuss. It has long been recognized that

the load should be so disposed over the body as to cause the least restriction of action not only of the limbs but also of the internal organs; and so balanced as least to disturb vertical equilibrium. These requirements have been variously met in different forms of equipment. As a rule the smaller accoutrements balance each other, and the main difficulty is the pack. This cumbersome load could in theory be taken either (i) on the head, after the fashion of the native carriers of many parts of the world, a situation giving excellent vertical balance, but impracticable for troops; or (ii) on the shoulder blades, and so widely diffused over the ribs; or (iii) on the small of the back, where there is a strong bony arch to support it. The great majority of armies have adopted the second alternative—the dorsal position; although as Colonel Mareschal has pointed out (article translated in the *Military Surgeon,* vol. xxi), this has three notable defects, namely a static error, in that the centre of gravity is displaced backwards and causes stooping; a physiological error, in that it reduces the respiratory capacity considerably (Zuntz and Schumberg showed a reduction in the vital capacity of the chest of eleven per cent under the load of our pre-war kit, fifty-nine pounds); and a military error, in that it interferes with the handling of the rifle. Such being the case it is not surprising that many armies are considering—and some have adopted—the lumbar position. Our own army has always kept to the dorsal position and has to a large extent eliminated the faults of earlier equipments, wherein the chest was compressed by cross belts, bandoliers, etc. Even so, the 1908 web equipment, excellent as it was in many ways, was far from perfect and induced considerable fatigue.

Experimental work in 1919 by Cathcart and Lothian (Report to War Office on *The Soldier's Equipment in Relation to Energy Expenditure*) indicated the lines on which a definite improvement in comfort and economy in energy expenditure could be affected. This modification involves the alteration of the pack support straps from the corners of the pack to the centre, after the fashion of a rucksack, thus enabling the weight to be taken by the large relatively immobile and solid muscles of the base of the neck, and avoiding the outwards displacement consequent on the swing of the arms, which formerly brought the weight bearing straps on to the thinly covered head of the humerus, and resulted in the sense of drag. By the adoption, further, of support straps from the front braces to the base of the pack, the latter has been really supported (after the fashion of a "couple") instead of, as before, merely suspended; and a very great and real improvement in comfort assured, with entire elimination of upward pull on the waist belt, or of constriction of the chest. Numerous investigations showed, moreover, that not merely does the new equipment have a more comfortable "set," but is carried much more economically as regards expenditure of energy on the march. Without going into details, it was estimated that a saving of seventeen per cent of the cost of carrying the 1908 pattern was effected. For a division on

service, marching say for two hours a day only, this is a saving equivalent to the potential energy contained in over 8,000 pounds of food, or over 1,900 field rations. Otherwise, where a man could march three miles with the former equipment, he can cover three and a half miles with the new pattern for the same expenditure of energy. But it would be rash to say that the perfect equipment has yet been discovered.

As to the question of a system dependent on better balance, it is interesting to find Parkes' observation that the old English packman could do thirty miles a day with ease carrying a pack of, often, over forty pounds, slung fore and aft. The milkmaid employs a similar device but with a lateral, right and left side, balance (*vide* Swiss milk advertisements). A study of the practice of native carriers has here a certain interest, but is not too applicable to military conditions. The natives of Baltistan (Lesser Tibet), the males of which are mostly professional carriers, carry a standard load of 60 pounds and frequently do their 12 to 15 miles a day over country of all gradients, and up to 3,000 feet. They march stooping, the weight poised on the shoulders, and employ a wooden apparatus not unlike a pickaxe with which to help themselves along, and on which the load is rested in situ to ease themselves every quarter of a mile or so. The Turkish hamal (porter) exaggerates this marching attitude, carrying immense loads, but attempting no great distance. In such cases, however, the march is the chief work of the day and rest is assured on its completion.

In general the soldier must assume an upright carriage on the march (other than the slight stoop of fatigue and compensatory stoop inevitable with a heavy dorsal load), but it is not yet proven that this is the most economical means of covering the ground. The French marche-en-flexion has much to recommend it, and an army of fine marching troops, namely the Turkish, have a peculiar loping action which is not far removed from it, and which called for the attention of the *Times* Military Correspondent (October 22, 1912).

The other factors affecting the march, important as they are in a complete analysis of the subject, hardly call for comment here, but it can never be forgotten that each of them plays its part. The halts to be taken—their frequency, duration, etc.—fall to be worked out scientifically; the questions of food and water supply, of clothing—its nature and cut—and footwear; of climate, and of the position of men as regards the column in which they are marching; the hours of the day in which the march is carried out; and, not least, the nature of the terrain over which the march is conducted; all of these have a bearing on the systematic study of the march in relation to health, but are hardly so intimately related to the question of load as to be worthy of present discussion.

V.—The Effect of the Load on Health.

By the middle of the nineteenth century, the growing habit of scientific inquiry among all men, and not least among the army doctors

of the period, had reached a stage when definite opinions on questions of the soldier's fitness, based on careful observations, could be offered for mature consideration and review. From this time we have a wealth of data as regards the soldier's load and its effect on his health, and in reality the first efforts to reduce to a sensible figure a load which was really unhygienically heavy. These matters, freely discussed after the Crimean war, merit brief notice.

It is to be recalled that the armies were no longer recruited to the same extent from sturdy yeomen, but more and more from the less physically fit urban communities which grew up as a result of the industrial revolution of England in the nineteenth century. The rigid oppression of the accoutrements and the compulsory carriage of a heavy total load resulted in heart and lung lesions among the less resistant soldiers, whose case was now brought more to notice than previously by reason of the growth of a national conscience, and a sense of responsibility towards soldiers broken in the wars. We are not surprised, therefore, to find the French army ascribing to their heavy accoutrements the high incidence of emphysema of the lungs, which had for some time been a matter of comment; and as a result an investigation and re-organization which reduced their equipment after the Crimea by some twelve pounds (Rossignol). (Experiment showed that over seventy per cent of men suffered from dullness over the base of the lungs after a heavy march.) In our own army similar observations led Dr. McLean (Professor of Military Medicine, Army Medical School, Netley) to investigate the frequency of heart and lung lesions among the troops with whom he was stationed and to announce the facts he elicited in a lecture to the Royal United Services Institution, and later before a Royal Commission appointed to investigate the whole question. McLean showed that of the discharges from the army of men under 20 years of age some 14 per cent were on account of heart lesions, a figure not increasing proportionally among men discharged over 20, although it is notable that heart conditions are more prevalent in proportion with age; while under 20, one man discharged in every three was on account of lung disease. These figures do not stand comparison with the civilian populace; one can exclude rheumatism and other contributory factors and still find a very high incidence of chest and lung disease among the more immature soldiers. This is not on account of their net exertion, which is no greater than a navvy's, but from the fact that their exertions are maintained, not with open necks and rolled sleeves like the navvy, nor in specially adapted costume like the sportsman, but at the utmost possible disadvantage as regards the weight carried and the entire arrangement of dress and equipment. These facts dominated the minds of the members of the Commission, who laid down what they considered a standard kit for the soldier, and whose views may be found summarized in one sentence: "The conditions of war demand that the marching powers and endurance of the soldier must not be lessened by unnecessary weight or a defective

mode of carrying it. *Ceteris paribus*, the army that is least weighted, and can move with the greatest rapidity, must have the advantage."

The amount of sickness in South Africa, much of which we now recognize as preventable, tended to obscure the extent of heart and lung disease in the troops who took part in that war; but the well recognized "soldier's heart" remained prevalent, and alteration in the equipment was again considered advisable.

When further we come to examine the results of the huge load carried in the recent war on the physique of the troops engaged, we find a tragic tale of wastage. From the earliest months of the war, the medical press was regularly exercised with the extent of heart lesions in military hospitals, not only among our own troops but almost equally among our allies (*vide*, for instance, the numerous articles in the French *Archives des Maladies du Cœur* relative to cardiac overstrain in soldiers, consequent on overwork attributed to marching with an over-heavy pack). Without going into detail, it may suffice to indicate the extent of such conditions by referring to the 120,000 *pensioners* on the books of our Ministry of Pensions on account of pure heart conditions attributable to the war, quite apart from those others attributable to or complicated by definite epidemic or infectious disease. The close connexion between the work involved in carrying the weighty equipment and heart conditions was obvious to any careful observer in the war. It was peculiarly evident in Macedonia, where men, debilitated by malaria and involved in marching in irregular mountainous country, developed heart lesions to such an extent as to call for remark and comment among the hospitals at Malta. The probability that the strain on the vascular system was also responsible for much of the so-called trench nephritis adds still further to the tale of broken men resulting from consistent overloading. In any case, such a state of affairs is obviously no economy, and justifies the careful attention that is now being paid to the whole subject of the military equipment, its weight and mode of carriage.

While in the ordinary way the march should not affect fit men injuriously, under certain circumstances it does, usually either (i) where troops are overloaded and undertake a single long march, especially if the contributory factors of adequate food and water supply are defective; such a forced march in hot weather has proved again and again a costly movement; (ii) where marches not in themselves unduly long are repeated for a considerable period without adequate rests; (iii) where special conditions of climate or pre-existing disease are found, e.g., in warm humid countries, or with troops weakened by disease, e.g., malaria. As instance of the latter case, instructive figures bearing on this question are given in the *Malaria Report of the British Salonika Force for 1918*. In this Report is described how during a period of four weeks of marching, the troops of certain brigades were studied as to the casualties on the march. The majority were infected with malaria, some highly so, and the latter were

marked off as "Y personnel." Some 9 per cent of the troops engaged became casualties during the period in question (as contrasted with 5 per cent during the preceding four weeks), but of the Y men no less than 22 per cent were affected; and the latter, although forming some 11 per cent only of the total personnel involved, contributed 25 per cent of the total casualties.

Apart from actual acute ill effects, such as heatstroke and derangement of the heat-regulating system, such hurtful conditions as the above may cause a lot of inefficiency not perhaps so noticeable at the time, but tending to cumulative injury as well as immediate exhaustion. The effects are mostly on the circulatory system and the lungs, and are provoked by the partial limitation of movement in the chest at a time when it requires all its freedom. We have already referred to the frequency of emphysema in old soldiers. More important probably are the effects on the heart and circulation, and the augmented blood-pressure. Lian and Binet (*Arch. des Mal. du Cœur*) have described well the typical overstrained heart case with rapid palpitating action, shallow and difficult respiration, and chest oppression. If such cases do not immediately break down, to add to the list of war cripples, the effects may wear off after a time, but the heart commonly shows the results; and the "soldier's heart" is a recognized clinical entity of long standing. Further, the probability is that deficient arterial circulation to the kidneys, and back pressure on these organs from the overloaded right heart and liver, are accountable for much of the nephritis which was so marked a cause of inefficiency in the war. Suggestive indication of the effects on the kidneys is given by a German observer, Rumpel (*Verh. Deutsch. Kongr. Med.*, Warsaw, 1916) who found the urine of over eighty per cent of men affected after a forced march under a load of eighty-eight pounds. The observation, of course, is no definite proof as to the cause of war nephritis, but indicates the physiological relationship between load and vital organs. Medical grounds thus confirm the need for a reduced load, and there is no need to back the statement with more detailed medical facts or statistics, which are out of place in this memo.

VI.—The Load to be Carried.

The soldier's load may be said to comprise (*a*) his clothing; (*b*) his offensive arms; (*c*) his defensive armour or equipment; (*d*) essential accoutrements; (*e*) "necessaries"; (*f*) personalia. These have all varied considerably according to the mode of warfare of the age, and will be discussed in the following sections: but in general we recognize that while certain offensive and defensive equipment, clothing and personalia have been common to all armies of all ages, the uniform and accoutrements have only been standardized for some two and a half centuries, the "necessaries" for about one century, while the item personalia has consistently diminished with improved regimental discipline and organization of the train.

There has been in the past pretty general agreement with Napoleon's dictum (originally delivered from St. Helena in criticism of Rogniat's *Consideration of the Art of War*) that the *essentials* of the fighting man comprise his arms and ammunition, trenching tool, knapsack, and four days' rations. Nowadays, however, there is a tendency to question the necessity of some of these items; and in estimating total load we must include the uniform and underclothing actually worn (one set) also as essential.

As regards the arms, no one would quibble. The amount of ammunition to be carried on the man, however, and the amount relegated to 1st line transport is a question which has been raised in many armies. While some would put 200 rounds on the man (Russo-Japanese War, 1904), others would reduce the number to eighty-eight (New trial French Equipment, 1908—v. *Jahres-berichte uber das Heer*, xxxv). This is actually a question primarily for the General Staff, but the fact remains that the principle of the carriage of a certain ammunition reserve in 1st line transport exists in many armies, and a further reduction of that carried by the soldier is, I understand, not unfavourably considered by others to-day. Of course, further relief of the individual soldier involves inevitable increase in transport, and this question of transport really dominates the whole situation.

As to the knapsack two schools of opinion exist; some consider that a man separated from his knapsack is little likely ever to see it again; and some would chance that as merely a casual misfortune. It largely depends on the contents and how far they are essential. A variety of campaigns has shown that troops are actually quite capable of doing without their knapsacks for periods of even weeks on end, as did, for example, the Prussian troops in the wars of 1866 and 1870 (*vide* the march of the 8th and 10th Corps on Paris), and as both Russians and Japanese did in 1904 (*British Officers' Reports on the Russo-Japanese War*). In the case of the Japanese, coolie transport was used to forward packs after the advancing troops, and if there were occasional periods of inconvenience from lack of them, it was considered a lesser evil than being handicapped by their weight in action. The Russians soon found that their load of fifty-six pounds "greatly reduced the mobility of their troops," and that "the men were unable to carry it, and march and fight" (Colonel Waters Report); this fact caused the Commander-in-Chief to direct that either greatcoats or kitbags should be carried in carts. In many instances, too, they were compulsorily left behind under the care of a small guard. In the later Ashanti Expedition also we wisely carried men's kits, and achieved rapid success. Wherever discipline permits of it, men tend to eliminate the pack of their own accord even without official order, as, for example, did the Confederate troops, *vide* the extract from *Stonewall Jackson*, quoted at a later part of this paper. Again, one might refer to the Turks who, commencing the war with Russia with a cumbersome official equipment,

had by the time of Plevna come down merely to a single large haversack capable of taking all that the owner thought necessary to take with him (von Herbert, *Defence of Plevna*). Under such circumstances, carrying only the haversack with eighty rounds, a water-bottle and a week's biscuit ration, one is not surprised to hear of their excellent march from Widdin to Plevna in eight days, an average of 14½ miles per day. It may be argued that undisciplined armies do not afford a just basis for comparison; but it should be recalled that in such armies the circumstances are usually less satisfactory and certainly less rigid than in disciplined forces, and work in the direction of the evolution of a sane and practical equipment.

On the other hand, however, it has long been contended that the risks of separating a man from his knapsack and small kit are unjustified; and that troops are more independent of the auxiliary services if fully self-sufficient. A favourable picture of troops serving in such conditions is painted by Ranken (*In Morocco with General D'Amade*), who ascribed the physical quality and high spirits of the French troops to the fact that they were independent of transport for their needs, carrying each a complete home on his back, and therefore avoiding the delays in feeding and housing so common on arrival of troops in camp. The question is one open to discussion. It may be that its solution will lie in the direction of assistance in transport of the pack during the earlier stages of the campaign, ere yet reservists and young soldiers have hardened to the physical ordeal of the march; and that a more easily detachable pack, such as, for instance, the admirable rucksack of the Gurkhas, may meet the case. The pack is the dominating item of the kit; the question of its necessity hinges on the amount of small kit, spares, and so-called necessaries to be carried.

Coming now to the rations we find another subject of controversy. Commencing the late war with one day's rations in reserve only, we soon found it necessary to add a second, in spite of the excellence of our supply services. Whether Napoleon's four days' supply will ever again be necessary owing to the drawing out of battles is uncertain, but the improvements of modern transport render it doubtful. It is, further, a common failing of soldiers to limit the rations carried in this way to a minimum. For instance, Stonewall Jackson's men were presumed usually to hold three days' reserve rations, but Henderson (*Stonewall Jackson*) affirms that they were consumed as soon as possible after issue; and the experience of many officers in the Great War will confirm that in this particular minor failing human nature has changed but little. Comparison with the past is not altogether fair inasmuch as the "ration" referred to has usually concerned the staple article only, viz., rusks (Romans, three to thirty days), oatmeal (Scots, one week), or biscuit (one pound per day, Cromwell, seven days) (see Firth's *Cromwell's Army*); whereas to-day an iron ration weighs almost 2½ pounds. Furthermore, the general and growing employment of field cookers makes it a matter open to considera-

tion whether much of the reserve insisted on by Napoleon cannot be carried by the regimental transport, and whether, indeed, there is still the same necessity for the individual soldier's canteen. "The latter may often be useless on account of lack of fuel, and absorbs time and energy; the cooker, on the other hand, keeps a company together by attracting stragglers and economizes energy; while if a detachment does get separated from it the men are no worse off than if they have canteens, but no fire. Single men always find something to eat, and, if not, that cannot come into consideration in so important a matter as lightening the infantryman's load" (Krauss, "Bekleidung der Infanterie," *Mil. Zeitschr.*, 1907, q.v., for full discussion; see also articles by Captain (now Lieutenant-Colonel) Dunbar Walker on "Mess Tins and Field Kitchens," *Journal of the Royal Army Medical Corps*, 1912).

So much for Napoleon's dictum; but that great commander did not know a war of high explosives and chemicals; and to-day we must add to the essentials a steel helmet, a box respirator, and, I should most decidedly say, a water-bottle. The latter item has long been a part of our British equipment, and, considering the variety of climates in which our army fights, a very necessary one. Cromwell's troops in England did not have one, but we find a general demand for them beginning about this time (*vide* the *Narrative of General Venables* and his call for them in the West Indies, 1655). It is evident, then, that the essentials alone reach a weight of about forty pounds.

In addition to these essentials, clothing, arms, and accoutrements, the soldier is equipped with a certain number of what are called *necessaries*. The term may for convenience be held to include spare clothing and extra small kit. It is here that there has always been the greatest loading up of the soldier's kit, and here that the greatest opportunity offers for cutting down his total load; but the very causes, social and hygienic, which led to its cumulation, will doubtless tend to resist its reduction. It is a remarkable thing that auxiliary troops, always less luxuriously outfitted than regular troops, have been as free from sickness as they have from load, and their flexibility and successful employment in the past has been a resultant of these two facts, despite the paucity of their "necessaries." Until the early nineteenth century it appears that the necesssaries of a soldier were left to his own discretion and provision, but were first standardized by Napoleon (*vide* the article in *Jahrbucher d. Deutsche Armee*, 1910). Early Clothing Warrants give indications of fairly liberal supplies of standard underclothing, e.g., the Warrant of 1844 allows three shirts and three pairs of socks; but this would appear to have been augmented considerably during the Crimean War owing to the sufferings of the Army from cold and exposure. (It was not quite so clearly realized then that ample body heat can be assured by a liberal and balanced ration, and that its free supply is at least as important as its banking-up underclothing.) The Royal Commission on Accoutrements went into the

question and suggested the following list, viz., 1 shirt, 1 pair of socks, 1 towel, 1 pair of boots or shoes, 1 soft cap, 1 holdall and 1 brush. We would now add to this list a toothbrush—and perhaps it would be as well to include a razor—although not truly an essential. The Greek troops have but one razor per Company; and our bearded troops after the Retreat from Mons neither looked nor fought any the less well by reason of their beards: (cf. also the French). Von Moltke, who advocated total abolition of the pack in favour of a lighter container, and who recognized as essential only the rifle, bayonet and 200 rounds, trench tool, water-bottle, and two days' rations, considered the following as necessaries: a light easy fitting uniform with open throat and a sleeved poncho cape; a spare pair of socks or footcloths and a pair of light shoes, an abdominal belt and spoon. His suggestion involved, of course, sending up warm underclothes or furs in winter.

There is much to be said for such reduced lists of necessaries. With improved communications and increasing rapidity of movement and transport it is probable that soldiers really would suffer far less from occasional and temporary shortage of any two or three articles than their health and efficiency actually do suffer from carrying too many things. It is beyond the scope of this paper, and is no business of the writer's, to go into a discussion of what the present-day soldier could actually get along with without detriment to his health, but it is fair to assume that much of what he carried in the late war could be kept in reserve behind him to be sent up as required. The genius for organization which made it possible for men to renew their clothing regularly at bathing and disinfestation centres, and with promptitude after mustard-gas attacks, should surely be able to cope with such a problem, more especially as mechanical transport multiplies from day to day its speed and forms. To give a man too many articles is often merely to let them accumulate dirt; and the scheme for regular bathing of troops in future wars might well be utilized to afford collateral assistance in cutting down much of the underwear the soldier actually carries on his person as spare. The general question of his hardihood is also involved, and many will agree that the soldier should be steeled and trained better to withstand the hardships and contingencies of war rather than that he should be overclothed in an attempt to give him greater bodily comfort. Not for nothing were the Spartans victors of old. In passing from the subject one may just mention that references in all literature point to the greatcoat as being one of the first articles to come under revision. As already stated, v. Moltke advocated a poncho; Jackson's men took to a blanket and waterproof sheet in lieu; our own Jackson (*The Formation and Discipline of Armies*) recommended a cloak as a substitute both for the greatcoat and the blanket, "giving a covering for the night," and being "all that a soldier requires for his comfort and the preservation of his health." Many of our own soldiers prefer the "Coat British Warm" pattern, while the Swedish "sweater" and sleeping

bag suggest an entirely different protection. Here we have a subject ripe for present experiment and trial.

In this section we must also include mention of the personal in belongings of the soldier. This is, among the Infantry of to-day, usually of comparatively low weight, and does not compare with the loot and personal belongings of soldiers of ancient armies. One may generalize by saying that the accumulation of souvenirs, etc, is in inverse proportion to the rigidity of regimental discipline; and in any case any excessive quantity is discarded when active movement compels reduction of the load. For the modern soldier some five pounds would appear to be an average maximum figure for this extra personalia, beyond which few would care to load themselves. Of this, probably ½ pound is smoking materials and accessories, and three pounds clothing and toilet accessories—the remainder usually printed or writing matter, souvenirs, and so forth.

But in ancient armies the personalia often bulked largely in the soldier's load. In the Napoleonic wars, for perhaps the last time, the soldiery still lacked that rigidity of discipline which nowadays prevents the free "acquisition" of non-military articles of value, and to an extent comported themselves, when facilities offered, after the fashion of the older armies, who were frankly encouraged to recruit and to conquer for the perquisites which would follow. Even in Puritan Cromwell's time we find that although he selected for his Ironsides primarily "freeholders' sons with consciences" (Clarendon, *Hist.* VI), nevertheless Sir P. Warwick was sufficiently cynical to note that "good pay *and plunder* constituted a natural member of Godliness."

In the Napoleonic wars we find frequent reference to the souvenirs, or, frankly, loot, which was so commonly an additional burden. The classic case of Serjt. Bourgoyne's knapsack is usually referred to as indicative of this extra load. Inclusion of the reference may perhaps be justified here by its general interest. In his *Memoirs* the Serjeant writes, on the occasion of his leaving Moscow on October 19, 1812: "I spent the time in making an examination of my knapsack, which seemed too heavy. I found several pounds of sugar, some rice, some biscuit, half a bottle of liqueur, a woman's Chinese silk dress, embroidered in gold and silver, several gold and silver ornaments—amongst them a bit of the Cross of Ivan the Great—at least a piece of the outer covering of silver gilt, given me by a man in the Company who had helped in taking it down. Besides these I had my uniform, a woman's large riding cloak (hazel colour, lined with green velvet; as I could not guess how it was worn, I imagined its late owner to be more than six feet high), then two silver pictures in relief, a foot long and eight inches high; one of them represented the judgment of Paris on Mount Ida, the other showed Neptune on a Chariot formed by a shell drawn by sea horses, all in the finest workmanship. I had, besides, several lockets and a Russian prince's spittoon set with brilliants. These things were intended for presents, and

had been found in cellars when the houses were burned down." This famous pack was carried for a month, and only lost the day after the battle of Krasnoe. It is evident that it contained all the spare clothing and rations and ammunition that the Serjeant possessed, and that the latter was of limited amount only (sixteen rounds). In other words, the extra load of unofficial articles was compensated for by a reduction of the official articles and "necessaries."

Similar references abound in the literature of the time, though few give such detail. Despite the improving discipline and control over the soldiery, plunder seems to have been still regarded as their fair perquisite, thus Scott (*British Army*) quotes the following description of the aftermath of Vittoria: "The booty captured was immense. Besides the baggage, horses, and other articles taken in the field, the value of the specie, plate and jewels was estimated at 6 millions of dollars: of this sum, only 100,000 dollars came to the military chest; the rest *was divided by the troops on the spot.*"

The subject need be carried no further; the practice is not now recognized; and we may now proceed to examine the load and armies of the past, to discover what was actually carried, and, where possible, how the load was borne.

VII.—The Greeks.

The Greek armies may be taken as examples of organized forces of the classical age, whose feats and records merit examination. With minor differences in equipment and in tactics, there was nevertheless considerable uniformity in the Greek armies; but of them all, the Spartans dovoted the greatest attention to ensuring an A1 physique and a thorough training both physical and moral, for their soldiers. In the earlier Achæan armies there was little organization, the whole tribes served together, and their camps were like villages (*Iliad* XIV, 30). In the historical period, however, the typical organization of cavalry and infantry gradually appears, the cavalry in turn disappearing (by the time of Marathon) and leaving infantry as the chief national arm. The most prominent type of Greek infantryman was the hoplite (fig. 1), a heavily armed and accoutred warrior whose peculiar genius was infighting, and who, from all accounts had not, nor was expected to have, any great mobility in battle; the phalanx did not depend for its success on mobility. As to the question of load, attention should be given to some of the less often quoted references if one is to ascertain the truth as to the weight normally carried by the man. Reference is often made to the load of the hoplite, on the assumption that his equipment was always carried by him, and that several centuries of war showed this could be done. Actually the case was very different, and the misconception has arisen through the prominence devoted to the hoplites *in battle*. Naturally, as the "Old Guard" in all major battles, and as the select representatives of their country, the interest of

contemporary and later historians was chiefly directed to them: but it is commonly overlooked that, on the march and when not actually fighting, every Spartan hoplite was accompanied by a helot who carried his shield and served him as attendant (*Xen. Hell.* IV). Historians describe, further, how the Athenian hoplites had each an attendant (therapon) who carried his arms and three days' rations (*Thuc.* III, 17 and *Xen. Anab.* IV, 2). The necessity for this is easily understood on detailing the arms referred to. In the case of the Athenian hoplite, the defensive armour comprised a casque, tunic, leather cuirass with imbricated metal leaves, leggings, and a large shield, while the offensive weapons included chiefly a lance and short sword; the whole estimated to weigh up to some thirty-five kilograms or eighty pounds (*Aristoph.*, *Acharn.*, etc.). That it required fully adult men and not immature lads to wield this equipment, let alone carry it on the march, is clear from the facts reported by Thucydides, who described the Theban and other troops of the Bœotian Confederation as wisely posting the young soldiers of 20 as archers, slingers, etc., and only some years later passing them into the ranks of the hoplites.

FIG. 1.—Greek Hoplite.

This leads us to mention the light infantry, which gradually assumed an increasing importance in the Greek forces. The Spartans gradually realized the value of arming their helots as slingers, javelin throwers and archers, and utilizing them as sharp shooters, scouts, and light infantry generally, but it was left to the genius of Iphicrates of Athens in the fourth century to revolutionize current ideas of the equipment and practice of war by his army of light infantry, who so heavily defeated and almost destroyed a Spartan force. This new infantry was equipped with a light leather-covered wicker or wood shield, a linen corset only, a lance

and a sword; and by their economy, suppleness and mobility gave a new significance to the value of light infantry (*Xen. Hell.* I). These men, we are told, were eminently useful as scouts, on the march, and were able to undertake duties hitherto impracticable for the weightier hoplites. From this point, lightly armed and defended infantry became generally organized, and one finds many references to the peltasts, corps of slingers, etc., who, by the time of the Peloponesean War, had become the recognized types of infantrymen, defended only by a light shield, and armed with their special forms of offensive weapons (*Thuc.* III). The weights they carried were apparently not great; the archers carried some twelve to fifteen arrows, the slingers a "pocketful of projectiles" etc. (*Xen. Anab.* V).

By this time, too, we learn of the very considerable baggage trains accompanying the armies, on the carts and led beasts of which were piled the cooking utensils, clothing and blankets, tents, rations and spare arms of the force. With the train went the numerous body of batmen, attendants, valets, etc.—a picture clearly drawn by both Xenophon and Thucydides of an army in which each individual soldier was not heavily encumbered with his own possessions, or even arms. Each day's march was normally followed by a day's rest, and the individual man was expected to forage for his own rations, with the aid of his ration allowance. Philip of Macedon, like Iphicrates, attached great importance to rapidity of marching and manœuvre, and to this end did much to make his army mobile by embodying not only heavy infantry, with long pike-lances, but a considerable force of light infantry (the hypaspistes). Indeed to his many corps of light troops must be ascribed much of his success. Still further to disencumber his army he reduced the baggage considerably (having learned how frequently the Greeks were paralysed by their trains), and allowed one attendant for each ten infantrymen only. At the same time he organized his commissariat on the expectation of living as far as possible on the country, and rarely carried much food (*Arrian* III, *Front.* IV).

VIII.—The Romans.

The Roman Army stands out as one whose exploits have been chronicled in considerable and meritorious volume; yet even here we find but parsimonious detail in the hard facts of internal economy. For the most part we are indebted for occasional references to Cæsar and other contemporary writers, and to the late *Epitome Rei Militares* of Vegetius A.D. 395—(in reality an appeal for the re-organization of the army of that date). From first accounts it appears that the Roman soldier of the Republic carried a weight in battle considerably in excess of what would nowadays be expected of a soldier. Statements as to the legionary's load are often, however, loosely made, and transcribed without examination; nor has attention been given to verify whether indeed in some of the more outstanding marches he actually did carry it all. One must frankly admit

that he showed extraordinary powers of endurance, powers frequently alluded to with pride by contemporaries, and contrasted favourably by Polybius with the less distinguished energy shown by the Greeks.

To the critical eye of competent judges there was "little to distinguish the Roman infantry from mules of burden" (Josephus), while even Vegetius' description recalls—

> "The Roman Soldier bred in War's alarms
> Bending with unjust load and heavy arms."
>
> (*Verg. Georg.* III.)

That the Romans were able, in their palmy days, to carry considerable loads, and to march quickly and well, was due principally to their mode of selection of recruits, and to training, more training, and still more training.

In the early days of the Republic, recruits were classed *and armed* according to a property qualification, only the first class being accoutred in what we now regard as the standard Roman equipment, the two lowest classes having no defensive armour at all. Later, Marius reorganized the whole army and armed all his legionaries alike. Augustus finally adopted the standing army of volunteer soldiers, thus overcoming the growing dislike of the Roman bourgeois for compulsory service; from this time the army gradually lost its distinctive Republican character and was recruited more from the provinces than from Italy itself. In the earlier days recruits joined most commonly at the age of 20 (*Corp. Inscript. Lat.*) and were selected from among the brawny peasantry primarily on a basis of sturdy strength rather than of stature (Vegetius). This point is of interest in view of the rigid recruiting regulations of our own times which base selection primarily on stature, and pay comparatively less heed to the general physique or body-weight.

The Romans chiefly relied on four months' training for transforming the recruit into a good soldier, and a hard and testing training it was. Detail is hardly necessary here (Vegetius gives it fully) but attention must be called to the importance laid on marching, as well as on all other forms of physical exercise. Realizing the value of a just correspondence of movement, the Romans insisted on an exact and vigorous cadence of steps in the march, such a step as would cover twenty Roman miles (each 1,666 yards) in five summer hours (i.e., $\frac{5}{12}$ of the hours of daylight in summer)—the ordinary march step or militare gradu: while for speeding up, twenty-four Roman miles had to be covered—the forced march or pleno gradu. I estimate these rates at some three and $3\frac{1}{2}$ miles per hour respectively. During his training the recruit was exercised with weapons weighing double the usual weight, and to prepare him for the burden of his eventual marching load, was "constantly exercised" in marching with a load up to a maximum of sixty Roman pounds, i.e., *forty-five pounds in present day units* (a libra = twelve ounces). This is important, and suggestive of its being *at least as great as that he was*

destined to carry when embodied (cf. his weapons). *Furthermore it is the only reference I can find to justify the very frequently quoted and misleading statement that the Roman legionary constantly carried a sixty pounds pack!* The legionary's load will be discussed below, but the point is important. The training load in an army which purposely stressed the rigours of the recruit is no sure criterion on which to base statements as to the regular soldier's pack, but is admittedly suggestive.

After his training the recruit was embodied in the legion, and posted according to his physique and aptitudes. He was still "constantly exercised." Vegetius tells us that thrice every month the legions were required to march ten miles out and back at the regulation pace, with intervals of quick marching, across broken and irregular country and in full kit (although it is questionable if the rations required to be carried in war were included in the kit). So trained and maintained, one gives willing credence to the many tales of fine marches by Roman troops. We may note that a soldier who showed special keenness in his work was promoted and given a double ration of wheat—a sure incentive to enthusiasm.

The clothing and equipment of the legionary comprised a woollen tunic, a leather doublet (often with metal facings), heavy sandals and a russet cloak. In cold countries he wore fasciæ (puttees), otherwise his legs were bare. For defensive armour he had a metal casque, a greave on his right leg, and a large half cylindrical shield of leather tipped with metal; and for offence, his sword (two feet) on the right side, dagger on the left, and two six feet javelin-pikes (pila) carried usually in the right hand. Apart from his arms, however, each soldier was, we are told, required to carry a certain equipment and rations for so many days. The equipment included variously a saw, spade, hatchet, cooking vessel, basket, chain, and stakes, and formed a bundle of awkward and cumbersome nature. A device in the nature of a forked supporting stick to be carried on the shoulder (like a tramp), and nicknamed after the originator Marius' mule was standardized to carry the personal equipment, which with the rations and other articles carried is often referred to comprehensively as the "sarcina" (cf. our pack). As to the weight of all these articles we have no exact knowledge. The two pila would certainly weigh 12 pounds, the shield and sword 10 pounds, the clothing, etc., 15 pounds, and the "pack" say as a maximum 45 pounds. Total sum, say eighty pounds. This is a very high figure, comparable to that carried by our infantry in France in winter during the later stages of the recent war, but it is quite credible and compatible with fairly effective *short* marches, in the case of a very well trained and hardy soldiery.

The figure is, however, open to criticism along several lines. The first is the ambiguity as to whether *every* soldier carried *all* of the axe, saw, etc., group quoted above, or only a selection. In view of the elaborate technical and engineering staff of the legion it appears more than doubtful; and I find myself completely in accord with Mommsen's view that they

were distributed here and there. As to the rations carried, very considerable differences are noted, depending naturally enough on the local circumstances—possibility of local purchase from friendly inhabitants, etc. The standard usually believed to have been laid down was seventeen days' rations (Sandy's, etc.), but others agree to three days' supply being the usual (Smith's *Dictionary of Antiq.*), while we know that Julian's men had twenty days', and Livy informs us that in his time thirty days' stock was carried. Here obviously there is a great margin of error. The basis of the Roman ration was wheat, issued at the rate of about thirty litres per month, say fifty-five pounds weight. Later on this was issued not as wheat but as rusks, weighing only three quarters of the weight of wheat. (Calculations in Daremberg's *Dict. des Antiq.* show that the daily bread equivalent was identical with that of the present day French infantryman.) Assuming a fourteen days' supply of such rusks to be carried, twenty-four pounds would be definitely included in the sarcina, while with a three days' supply some five pounds only are accounted for. I cannot find it agreeable to accept a forty-five pounds "sarcina" (and no other figure is ever quoted) with the most prominent content varying in such a way; and am inclined to reduce the eighty pounds total accordingly. Probably the forty-five pounds to which the recruit was trained was a more accurate figure of the *total* load (but see below). As a matter of interest it may be noted that while cereals comprised the stand-by of the ration, the soldier had meat as often as possible ("at least once a day"—*Strabo.* VIII), usually pork. Lard, salt and oil are frequently mentioned in lists of military stores, and we know that they were carried on the train. Such being the case it is a fair assumption that a very large part of the soldier's rations were also carried in bulk on the train. Vinegar and water (posca) formed the universal drink, a peculiarly interesting empirical use of antiscorbutics, the use of which appears on occasion to have been much needed after a prolonged period of living on rusks only (*vide* Seneca's description, *Ep.* 83, of getting sick and losing his teeth).

Mention of the train, however, leads us to the most interesting commentary on the fabled strength of the Roman soldiers, and leads to a revelation of the fact that—as in the Greek Armies—there was a very considerable assistance given in the carriage of heavy articles by the transport and personnel of the train. Why this is often overlooked is doubtless because the Roman historians, regarding the non-combatant train as of secondary interest only, but rarely mention it in detail, and concentrate on the feats in battle of the legions. That carts and pack animals were used in this way to relieve soldiers on the march (in addition to their proper functions) we know from various sources (*Vita. Alex.* 47, *Suet. Calig.* 43, etc.). Even in rigid Cæsar's time we have reference (*Bell. Gall.* VII, 47) to the *magnum numerum impedimentorum et muliorum.* Not less important than this assistance from animal transport however was that afforded by human assistants, the calones (attendants), slaves, and

others referred to by Cæsar (*Bell. Gall.* II) occasionally becoming so numerous a swarm (*multitudo servorum—Bell. Afr.* 74) as to outnumber the legionaries (*Tacit. Hist.* II 87). To deal with these individuals they were organized in groups of 200 under non-commissioned officers and marched with the train. Many being slaves, can there be any doubt as to their masters utilizing them to carry at least the non-effective portions of their equipment on the march (*vide Cæs., Bell. Civ.* III)? When Sulla besieged Athens, to such an extent had this portion of the army grown that 20,000 muleteers are recorded as following the troops (*Pluto, Sulla,* 12). Finally, one must not omit mention of the women, who followed their soldiers (*Appian, Hist.* 85, etc.). It will be seen then that, while exact data are lacking, evidence exists to show that (1) the Roman soldier was *equipped* with a fairly weighty and exhausting kit, but that (2) there is reason to suppose that he himself rarely carried it all on the march.

Confirmation of these ideas is obtained by a careful study of Roman sculpture, best of all from the reliefs on the magnificent Column of Trajan, also from that of Marcus Aurelius, the Arch of Severus and others. (*Vide* the photographs by Fröhner and Cichorius, whose monographs may be seen in the British Museum.) We find the carts and led beasts of the train loaded with warriors, shields and warlike gear; while a critical review of the 113 odd plates reveals only two occasions on which the legionaries carry anything other than their arms and armour. In one plate, among a section of twenty-five men on the march, one only carries a pick and one an axe. In the other, showing troops on the march, the men are carrying their "Marian Mules"—the forked support—on the end of which are their sarcinæ, or bundles of personalia. These bundles—principally food and utensils connected therewith, comprise the reserve stock of rusks, canteen, small pot, water bag, etc., and obviously do not weigh over fifteen pounds at the outside. The assistance given in carrying the sack of spare clothing, and loot, shield, etc., by the personal attendants or calones is also evident in a fragment preserved in the Louvre (Paris). Rough sketches of these reliefs are appended—fig. 2. From the combined evidence of writings (e.g., *Martial,* IX, 57 and even *St. Paul, Eph.* VI) and of the existing reliefs we may therefore assess the Roman's load, on the march, as about 50 pounds (35 pounds for clothing, arms and armour, and 15 pounds sarcina).

Those occasional examples of forced marches by the Romans, so often referred to as the normal, do not stand close analysis. For instance, the much quoted march of Claudius Nero (6,000 infantry), who covered 240 miles in seven days, was organized by messengers in advance in such a way that all the available animal transport of a friendly and enthusiastic populace was at his disposal. Under such circumstances an army may double its normal march, as for example did Napoleon's Imperial Guard on the march from Paris in 1806; in this case, by the use of carts in which half the men rode while the others walked, 435 miles were covered in

thirteen days, a rate and distance rarely equalled. On the whole the Roman system—inevitable with their equipment and easily understood—very wisely was *festina lente*. In the ordinary way the Romans do not appear to have made notably striking marches, and we learn, for example, that even the veteran force which invaded Britain covered only some

Baggage train.

Legionaries carrying sarcina on the "Marian Mule." (After relief on column of Trajan.)

Calo bearing his master's load. (After fragment in Louvre.)

FIG. 2.—Roman troops on the march.

seventy miles in the first seven days' marching, a figure comparable to "standard" marching but in no way extraordinary. These soldiers were quite unable to pursue their lightly armed opponents, clad only in skins and stripped for the fray, by reason of their heavy armour (*Cæs., Bell. Gall.* V); and from this time onwards we find the Roman soldier gradually less able to withstand the more mobile barbarian hosts and finally succumbing

before the Goths and Huns, against whom, Vegetius states, they were almost defenceless. The necessity for lightening the equipment of the troops was in this way borne in on the Romans themselves; the soldier apparently refusing to carry such loads and requiring finally in Valentinian's time a reorganization of the whole military fabric. But more important still was the increasing employment by the Romans of light infantry,

Light auxiliary. Heavy armed legionary.

FIG. 3.—Roman Soldiers.

unencumbered with heavy defensive armour, and able to act as skirmishers, scouts, and pursuit troops. These light troops and auxiliaries were evolved in like manner to the Greek peltasts and appear to have been equally efficacious; but could not of themselves do more than stave off the gradual deterioration and disorganization of the army. The contrast between the legionary and one of the light troops is indicated in fig. 3.

(*To be continued.*)

PART I.—*continued.*

THE INCIDENCE AND ÆTIOLOGY OF MALARIA IN MACEDONIA.

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Late Temp. Colonel Army Medical Service.
Wellcome Bureau of Scientific Research.

(*Continued from p.* 192.)

EXPLANATION OF THE LATE APPEARANCE OF MALIGNANT TERTIAN MALARIA.

Various suggestions have been put forward to explain the early occurrence of *P. vivax* and the late appearance of *P. falciparum.* One has been that *P. falciparum* requires a higher temperature for development in the mosquito than *P. vivax.* In Macedonia the temperature is sufficiently high for the development of *P. falciparum* long before the great influx of these cases commences. The difference in the temperature required for the two parasites is so slight, that this explanation is insufficient. Undoubted primary cases of malignant tertian malaria occurred as early as the beginning of May.

Roubaud[1] found that *P. vivax* developed more rapidly in *A. maculipennis* than *P. falciparum*, and he sees in this difference an explanation of the early appearance of *P. vivax*, which, as it were, obtains the first innings. It is only later that the more virulent *P. falciparum* is able to score and overtake it.

It seems to me that there is a much simpler explanation. In the case of benign tertian malaria the type of infection produced is undoubtedly a relapsing and quinine-resisting one, which tends to be of long duration, so that cases which arise in one summer carry over to the next. With malignant malaria, on the other hand, though the individual attack may be more severe, the cases are more amenable to quinine treatment and there is little tendency for one season's cases to persist. This statement is amply borne out by all the experiences of the war. The number of malignant tertian infections noted in troops returned to England is very small compared with the number actually diagnosed in Macedonia. It is the benign tertian infection which persists and relapses, and not the malignant tertian. At the height of the malaria season, as we have seen, the numbers of benign tertian and malignant tertian cases are approximately equal. There is then a gradual falling off of both types but much more so for the latter till for the period February to May blood film examinations only rarely reveal

[1] "Recherches sur la transmission du paludisme par les anophèles français de régions non palustres." *Ann. Inst. Past.*, September, 1918. Tome xxxii, p. 430.

P. falciparum. At the commencement of the mosquito season (May onwards) there is a very much larger number of carriers of *P. vivax* than of *P. falciparum* so that a much greater number of mosquitoes becomes infected with the former than with the latter, and the first marked rise in malaria will be due to benign tertian infections. Cases of primary malignant malaria, however, occur at this time, for a fatal one was seen at Labanah in the beginning of May, 1918. Experiments conducted with *A. superpictus* and *A. maculipennis* (see below) seem to indicate that both these mosquitoes are more readily infected with *P. falciparum* than *P. vivax* and that quinine has a greater influence in benign tertian cases in rendering them non-infective to mosquitoes than in malignant tertian cases; in other words, the gametocytes of *P. vivax* are more susceptible to quinine than those of *P. falciparum.* Both these factors would tend towards the more rapid dissemination of *P. falciparum.* Furthermore, the malignant tertian malaria in man develops more rapidly towards a large infection and produces in a shorter time a greater number of gametocytes than the benign tertian malaria.[1] It is not uncommon to encounter individuals in whose blood very large numbers of crescents are present without there being any acute symptoms. In benign tertian cases such large numbers of gametocytes are not seen and gametocytes, when present in any number, are generally associated with sufficient asexual forms of the parasite to make the individual acutely ill.

All these factors would tend to a gradual equalization of the number of mosquitoes infected with *P. vivax* and *P. falciparum,* or even to a reversal of the conditions which existed at the commencement of the malaria season. As I have already stated, no evidence could be obtained that any particular species of mosquito was especially associated with one type of malaria.

A very important result of the difference between the two types of malaria is that it should be easier to eradicate *P. falciparum* from a population than *P. vivax.* The former, as we have seen, is more severe while it lasts but has a natural tendency to disappear before the next malaria season. Furthermore, it is much more easily got rid of by quinine than *P. vivax.* Hence a population which took quinine during the non-malaria season would have its *P. falciparum* very largely eradicated and in time perhaps completely so. On the other hand, the more resistant *P. vivax* would be affected to a much smaller extent.

Severity of Macedonian Malaria.

The type of case seen in Macedonia does not differ from that of other malarial countries. The very worst cerebral cases do not differ clinically from the similar cases occurring in the bad malarial districts of China, India,

[1] James, in his recent book on "Malaria at Home and Abroad," makes a similar statement on p. 12.

Africa and elsewhere. It is true that a larger number of these cerebral cases appeared and had to be dealt with in a single area than has ever been the case before, but this is the result of exposing to infection a very large number of susceptible individuals, and is no indication that the Macedonian malaria is more virulent than that of any other country. The result would have been the same if the Army had occupied any other highly malarious country.

The high case mortality of 1916 (1·01 per cent) was undoubtedly due to hardships, exposure and unpreparedness under war conditions and similarly the much lower mortality of 1917 and 1918 (0·37 and 0·31 per cent) would have been still further reduced if ordinary peace time comforts and precautions had been available. If in Macedonia it appeared that the malaria was specially severe it was the result of life under war conditions and was not due to any special or peculiar feature of the malarial parasites of the country. The mortality rate followed closely the incidence rate of malignant tertian malaria, as shown by blood film examinations.

Latent Malaria.

There have been many instances of individuals who have had their first attack of malaria several months after any possible exposure to infection. Often the first attack has occurred after return to England. This delay in the appearance of definite symptoms seems to indicate a natural resistance to the disease. In some cases infection must take place without ever being recognized. These latent infections may reveal themselves only after some special exposure and they undoubtedly account for at least the majority of the cases which were supposed to have acquired malaria during the winter months.[1]

One instance of this latent malaria may be mentioned. The individual arrived in Macedonia before the malaria season of 1917. Prophylactic quinine was taken daily for about two weeks and then was abandoned. Part of the summer of 1917 was passed on the Hortiak plateau and the remainder of this year and the whole of the next in one of the most mosquito-free districts of Salonika. In February, 1919, the person returned to England, and two months after arrival a typical and severe attack of malaria with rigor, fever (105° F.) and sweating occurred. The last possible exposure to infection was at the end of the summer of 1918, at least six months before the attack. Absolutely no quinine had been taken since the beginning of 1917, and there had been no question of any malaria previous to the first attack in England, when a blood film showed an infection of *Plasmodium vivax*. Quinine ten grains three times a day was

[1] James ("Malaria at Home and Abroad," p. 109) mentions instances of latent malaria. He states that in these cases there has been an infection with "only a small number of sporozoites." It seems to me that the evidence for this is wanting. Is it not more probable that the latency depends on some natural resistance to infection?

taken for a month and then quinine more irregularly till it was finally stopped. There has been no recurrence for over a year, so that the malarial infection in this case is represented by the single isolated attack. Many of these cases occurred but this one has been mentioned as from personal knowledge I can vouch for the accuracy of the details.

Blackwater Fever.

This condition as it occurred in the Macedonian army has been dealt with fully by Colonel Phear[1] and other writers,[2] and I will content myself with a few remarks. As was to be expected with so much malaria in Macedonia, blackwater fever was not an uncommon disease. One result, at any rate, of the blackwater fever experiences of this war has been to emphasize more completely than ever its close relationship to malaria, and to establish beyond doubt, despite Gaskell's[3] remarks to the contrary, that it is something other than a mere quinine poisoning. On many occasions cases of blackwater fever were treated throughout with large doses of quinine given either orally, intramuscularly or intravenously, without any untoward result, so that it is clear that the drug can be administered in this disease without danger. A certain number of cases, however, commenced immediately after a large dose of quinine, just as in some cases an attack of malaria was brought on in this way. I remember a case when an intramuscular injection of 20 grains of quinine was about to be given to a man. He said, "If you give me the quinine, I will have blackwater fever; I have had it before." His prophecy proved correct, for a definite mild attack occurred within the next twenty-four hours. The man was a known malarial subject, but one may ask was this a case of blackwater fever or was it quinine hæmoglobinuria, a definite case of which is mentioned by Colonel Phear.

All grades in severity of the disease occurred in Macedonia from very mild cases which, with no symptoms other than those of ordinary malaria, passed blackwater for only part of a single day, to the most severe and fatal types with all the well-known features of the disease. It has always appeared to me that there is too great a tendency to regard blackwater fever as a condition quite different from ordinary malaria. In the usual simple attack of malaria there is a blood destruction due to the breaking up of both infected and uninfected red blood corpuscles. Hæmoglobin

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[1] Journal of the Royal Army Medical Corps, vol. xxxiv., January, 1920. In the year ending October 31, 1918, there were 136 cases of blackwater fever with thirty-six deaths.

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liberated is dealt with by the liver. In more severe malarial attacks the liberated hæmoglobin is greater, excess of bile is formed, and the patient becomes jaundiced. In some of these cases where the hæmoglobin liberated cannot be entirely dealt with by the liver it is excreted by the kidney, and a transient condition of blackwater fever results. When blood destruction is very great the liver can only deal with a small part of the hæmoglobin, and the kidneys attempt to excrete the rest. They are damaged in the process and all the typical signs of blackwater fever appear.

The object of these remarks is to suggest that in blackwater fever the same blood destruction takes place as in an ordinary malarial attack, but to a greater degree, and that the severe symptoms of the condition are due directly to the excess of free hæmoglobin acting on the body generally and especially on the kidneys, and not to any toxin peculiar to the disease. The true explanation of blackwater fever will be found in the method of blood destruction which takes place in an ordinary malarial attack.

Another point of interest was the common occurrence of malaria parasites in the blood during the blackwater attack. It became the rule to administer quinine during the attack if parasites were present. In some cases a typical malarial attack with the appearance of parasites took place during the course of the disease.

The type of parasite seen in blackwater fever cases was the usual malignant tertian parasite. Occasionally benign tertian parasites were seen but there was not sufficient evidence to absolutely exclude *P. falciparum.*[1]

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[1] In this connexion a paper by E. Hasell Wright (*Indian Medical Gazette*, May, 1920) is of interest. The author figures in a series of plates of over 650 drawings the different forms of parasite he has seen in blackwater fever cases and attempts to demonstrate that the infections represent several species of the malignant tertian parasite together with a piroplasma. I may say at once that all the forms illustrated are perfectly familiar to me in ordinary malignant tertian infections. The fallacy of the paper lies in the assumption that a parasite like *P. falciparum* has a hard and fast morphological structure. It is a very amœboid organism and when examined in the fresh state exhibits very active movements, so that an infinite variety of shape and structure is to be expected in stained preparations. Variations in size are also noted, as in the case of *P. vivax*, and these are probably dependent on variations in the growth resulting from irregularities in nutrition. It is not remarkable that the younger stages of *P. falciparum*, which consist of cytoplasm and chromatin and where pigment is absent, should often assume the shape and form of the piroplasmata, which consist of the same two substances. Any one who has examined films of the various piroplasmata must have been struck by the resemblance of certain forms to the parasites of malaria and I have often remarked that it would have been impossible to make a diagnosis on individual parasites. And conversely, in examination of films of malaria parasites one has noted the resemblance to piroplasmata. The fact that the malaria parasite with its infinite variation sometimes becomes pear-shaped is no proof that one is dealing with a mixed infection of *P. falciparum* and an unknown piroplasma. It seems remarkable that amongst the great number of illustrations the writer does not

TREATMENT.

I do not propose to discuss the treatment of malaria at any length. This is done fully in Major Anderson's paper below. Almost all conceivable forms of quinine administration have been tried and it would appear that the sum total of all the results indicates that for the handling of the bulk of malaria cases, whether due to *P. vivax* or *P. falciparum*, a course of oral quinine followed by week-end quinine in moderate doses gives the best results. Other forms of treatment by intramuscular or intravenous injection of quinine are to be reserved for special cases where gut absorption is not taking place, where the temperature for some reason does not respond to oral quinine, or where very rapid action is required in cerebral cases. This treatment does not bring about a sterilization of the patient of his parasites, but it reduces his fever and assists him over his attack till the natural powers of resistance bring about a cure. It is the method of treatment which, with various slight modifications, was in vogue in most malarial countries before the war. The enormous experience of malaria in the various campaigns appears to have added little new knowledge and has only served to show that no amount of juggling with the doses of quinine will disclose a treatment which is of outstanding superiority.

Quinine is a drug which was discovered accidentally. It is undoubtedly a specific in its action on the malaria parasite, but not a perfect one. It should be taken as a basis for experimental work which will lead to the discovery of some more potent derivative. Quinine can only be the first, and not the last, on the list of a long series of chemicals which will be found to have an action on the parasite of malaria. It seems to me that it is only along chemical lines that future investigations will improve the quinine treatment of malaria.

James ("Malaria at Home and Abroad," p. 182), says that perhaps the frequent failure to "prevent recurrences is due less to the inefficiency of the drug than to our ignorance of the correct way in which to use it." Here again this statement conveys the impression that quinine is a perfect drug, and that all we have to do is to find out how to employ it. There is undoubtedly a best way of administering it, but there is no reason to suppose that any particular method of administration will give infallible results.

HISTO-PATHOLOGY.

Examination of post-mortem material from cases of malaria has only emphasized the resemblance of the Macedonian disease to that of other

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figure any of the bridge forms of *P. falciparum*, nor are the accolé forms definitely depicted. It appears far more probable that *P. falciparum* has a greater range of morphological variation than is generally recognized than that such variations indicate a multiplicity of parasite.

parts of the world. Colonel Dudgeon and Captain Clarke[1] published full accounts of the histo-pathology of malaria, and brought out the interesting point that fatty degeneration of the heart and other organs occurs in malaria, and that this condition will account for some of the clinical symptoms observed in the disease. In another paper[2] they have attempted to co-ordinate the pathological appearances in cerebral malaria with the clinical symptoms.

It is unnecessary to enter into any detailed description of the well-known pathological findings, but there are one or two points to which attention may be drawn.

In material I have examined from cases which have died of cerebral malaria the most striking feature has been that histologically they fall into two main groups. Firstly, those in which the capillaries of the brain and other organs, notably the intestine, pancreas and spleen, are completely blocked with the well-known emboli of infected cells, and secondly, those in which very few infected cells could be found. Both these types had died with cerebral symptoms. Whether it is possible to distinguish these groups from one another before death I do not know, but on post-mortem examination they are clearly defined. The difference in the two groups must show itself in the effect of treatment. With completely blocked capillaries in which no circulation is taking place it would seem impossible for quinine, even injected intravenously, to reach the parasites. This stoppage of the circulation in the smallest vessels is complete. In the vessels slightly larger it is seen that while the walls of the vessels are lined with infected red blood corpuscles the central part of the lumen contains red blood cells only some of which, or sometimes none, harbour parasites. It would appear that in these vessels the blood is still circulating in the centre of the lumen, though the calibre is reduced by the lining layers of infected cells. In the larger vessels the circulation is still intact, for they are still clear, and infected cells are not adherent to the vessel wall. This gradation from the smallest completely blocked capillaries to the larger vessels which still have a clear lumen, is possibly dependent on the rate of flow of the blood. When the condition of complete blocking is produced quinine cannot reach the parasites unless by diffusion through the vessel wall from the lymph around it. In these cases it is difficult to imagine how quinine can possibly bring about recovery, and it is doubtful if it ever does, for the condition has been found after very intensive quinine treatment.

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[1] Dudgeon (Leonard S.) and Clarke (Cecil). "A Contribution to the Microscopical Histology of Malaria as occurring in the Salonika Force in 1916, and a Comparison of these Findings with certain Clinical Phenomena." *Lancet*, 1917, August 4, pp. 153-156.

[2] An Investigation into Fatal Cases of Pernicious Malaria caused by *Plasmodium falciparum* in Macedonia." *Quart. Journ. of Med.*, July, 1919, pp. 372-390.

In the second type of cerebral case, where post mortem no blocking of the vessels is observed, and in fact very few parasites may be discovered anywhere in the body, the chances of successful treatment are much greater. It may be urged that in these cases the capillaries were originally blocked and that the parasites had been destroyed by quinine. There is, however, plenty of evidence, such as the small amount of pigment in the spleen, to show that cases of this type have died when no quinine, or very little, had been given, and that they had no overwhelming infection as in the first group. It would seem to me that these cerebral cases, in which recovery has taken place after intravenous quinine, really belong to the second group, and that recovery of cases of the first type is practically impossible. What is actually the cause of death in the cases of the second type, where parasites are few, it is difficult to say, but in some of these at any rate capillary hæmorrhages into the brain tissue have been observed which might well account for the symptoms. In these it is possible that the administration of quinine in large doses would aggravate rather than improve the condition.

One other point calls for note, and this is the effect of intramuscular injections of quinine. It has been shown by Colonel Dudgeon that necrosis of muscle always takes place at the site of an intramuscular injection, and for this reason he thinks that intramuscular injections should be discontinued, or at least practised on a much smaller scale. It is unquestionable that in many cases of malaria, in which oral quinine for some reason fails to control the fever, one or two intramuscular injections will succeed, and that after this oral quinine seems to act more readily. That the necrosis at the site of an injection is very startling, no one who has seen it will gainsay, but, as the proof of the pudding is in the eating, so the intramuscular injection of quinine must be judged by the results. That it controls the fever in some cases when ordinary oral administration will not is undoubted, and, furthermore, the after-effects of the necrosis are practically negligible in view of the fact that so many thousands of such injections were given in Macedonia without any permanent ill-effect. The administration of two intramuscular injections each day for twelve days in the intensive course of treatment used at one time during the war no one would now entertain, for the results obtained were in no way better than those following more moderate measures and the extensive necrosis which supervened would not justify a continuation of this method.

It is, perhaps, not out of place to mention here that the medical arrangements for the care of the sick, so many of whom were malaria cases, in 1918 reached a very high degree of efficiency. Diagnosis was seldom in error, and treatment was carried out in a thoroughly scientific manner. Compared with the French, we were at a distinct advantage, for we had a larger number of medical officers and nursing sisters to care for our patients than they had. This difference is perhaps reflected in such

figures as these: in the six months, January to June, 1918, the British mortality for malaria was 0·7 per 1,000, while the French figure for the same period was 6·4. Great credit is due to all the medical officers, whether in hospitals or with units, or in the pathological laboratories, who quickly and enthusiastically acquired the necessary knowledge to deal with a disease with which the majority had had no previous experience. It is difficult to see how the medical arrangements could have been improved to any appreciable extent, and this satisfactory condition of affairs was due primarily to the great energy and unceasing interest of our Director of Medical Services, Major-General Sir Maurice Holt, K.C.B., K.C.M.G., D.S.O.

For a campaign in a highly malarious country it seems to me that the following equipment should be at once available:—

(1) Mosquito nets of two types: (*a*) Hospital nets of rectangular pattern with a calico strip round the base. (*b*) Bivouac type, in such numbers that each man may have a net at once and have it replaced immediately when damaged. If phlebotomus is known to occur, the mesh should be small—at least twenty-two holes to the linear inch.

(2) Some form of standard mosquito-proof hut similar to those used in Salonika in 1918. They were made of wood, canvas, and copper gauze, and can be quickly put together. There should be a definite scale for the supply of these. They can be used as mess-huts for men and officers, and as hospital wards in place of tents, offices, etc. They protect, of course, against flies as well as mosquitoes.

(3) Copper gauze to mosquito-proof buildings.

These are far and away the most important, though minor appliances should not be forgotten. Such are:—

(4) Mosquito boots, especially for officers and sisters.

(5) Mosquito veils and gloves. The value of these is very limited.

(6) Special clothing for the men, as, for example, turn-down shorts.

(7) Some form of mosquito swat to enable men to kill any mosquitoes found in the rooms or tents in the morning.

(8) Repellents, though these are of very limited use.

(9) Sprayers and fumigators.

(10) Pamphlets of a simple kind explaining the dangers of the mosquito and the reasons for the measures taken.

In addition, there should be a staff consisting of experts whose sole duty it is to report at once on the malaria incidence and dangers in the various districts to be occupied and to suggest measures of prevention.

Experiments on some of the Conditions Affecting the Development of Malarial Parasites in Macedonian Anophelines.

Numbers of feeding experiments were conducted with *A. maculipennis* and *A. superpictus* with a view to testing their relative infectibility with *P. vivax* and *P. falciparum*. Others were carried out to test the effect of cold on partially developed cysts and the possibility of an arrested development being continued when temperature conditions became more favourable.

Most of the mosquitoes used were collected at Lahanah or at Sacavca in the Struma valley. An error was thus introduced, as a previous infection could not be excluded. The very low incidence of infected mosquitoes, as described above, reduced this possible error to a minimum and, as a matter of fact, no previous infection in any of the mosquitoes used appears to have been present, as the size of the cysts and the stage of their development in the infection of the salivary glands always corresponded with the number of days which had elapsed since the mosquitoes had fed on infected blood.

The experiments were conducted during the summer and winter of 1918. The mosquitoes were either kept at the ordinary laboratory temperature or in an incubator or ice-chest, as the experiment required. They were kept alive after feeding on infected blood by allowing them to suck a solution of sugar in water on a piece of cotton wool placed on the mosquito net covering of the jar in which they were kept.

The maximum and minimum temperatures were taken every day.

All the cases on which the mosquitoes fed were known to have gametocytes in the blood. Most of the cases were taking quinine at the time of experiment but, as will be seen, this appeared to affect *P. vivax* more than *P. falciparum*.

Where mosquitoes hatched from pupæ in the laboratory were used this fact is mentioned.

The following experiments were carried out:—

Experiments with *P. vivax*.

(*a*) Mosquitoes fed entirely on cases taking quinine.

(*b*) Mosquitoes fed at least once on a case not taking quinine.

Experiments with *P. falciparum*.

Effect of cold after partial development (first experiment).

Effect of cold after partial development (second experiment).

Experiments with *Plasmodium vivax*.

A.—*Mosquitoes fed entirely on cases taking quinine.*

(1) July 4, 1918: One *A. maculipennis* fed on benign tertian case. 10th: Second feed on another benign tertian case. 13th: Dissected—no infection. Temperature of experiment: Average maximum, 30·2° C.;

average minimum, 21·9° C.; absolute maximum, 30·5° C.; absolute minimum, 21° C.

(2) June 30: Three *A. superpictus* (hatched in laboratory) fed on benign tertian case. July 3: Dissected—no infection. Temperature of experiment: Average maximum, 29·5° C.; average minimum, 22·3° C.; absolute maximum, 28° C.; absolute minimum, 21° C.

(3) August 5: Two *A. superpictus* fed on benign tertian case. 9th: Dissected—no infection. Temperature of experiment: Average maximum, 33° C.; average minimum, 24·2° C.; absolute maximum, 33·6° C.; absolute minimum, 23·6° C.

(4) September 17: Five *A. superpictus* fed on benign tertian cases. 19th: Second feed on another benign tertian case. October 1: Dissected—four *nil*, one with sporozoites in salivary gland—a possible previous infection as no cysts were seen, but the period of development at the temperature of the experiment would leave one to expect the glands to be infected. Temperature of experiment: Average maximum, 29·7° C; average minimum, 21·4° C.; absolute maximum, 31·7° C.; absolute minimum, 20·5° C.

(5) September 17: One *A. superpictus* fed on benign tertian case. 19th: Second feed on same case. October 1: Dissected—*nil*. Temperature of experiment: Average maximum, 29·7° C.; average minimum, 21·4° C.; absolute maximum, 31·7° C.; absolute minimum, 20·5° C.

(6) September 20: Two *A. superpictus* fed on benign tertian case. 21st: Second feed on same case. October 1: Dissected—*nil*. Temperature of experiment: Average maximum, 29·8° C.; average minimum, 21·5° C.; absolute maximum, 31·7° C.; absolute minimum, 20·5° C.

In these experiments one *A. maculipennis* and twelve *A. superpictus* were fed on benign tertian cases, taking quinine, with negative result.

B.—*Mosquitoes fed at least once on a case not taking Quinine.*

(7) May 30, 1918: Three *A. maculipennis* (hatched in laboratory) fed on benign tertian case (taking quinine). June 6: One dissected—*nil*. 7th: Two *A. maculipennis*, second feed on another case (not taking quinine). 15th: Dissected—both positive; two generations of cyst, one half grown and one with sporozoites; salivary glands with sporozoites. Temperature of experiment—laboratory temperature, about 18°—25° C.

(8) July 1: One *A. maculipennis* fed on benign tertian case (taking quinine). 10th: Second feed on another benign tertian case (not taking quinine). 13th: Dissected—stomach with pre-sporozoite cysts. Temperature of experiment: Average maximum, 30·5 C.; average minimum, 22·2° C.; absolute maximum, 33° C.; absolute minimum, 21° C.

(9) August 18: One *A. maculipennis* and three *A. superpictus* fed on benign tertian case (not taking quinine). 19th: Second feed on same case. 20th: Third feed on another benign tertian case (taking quinine). 21st: Fourth feed on another benign tertian case (not taking quinine). 22nd:

Fifth feed on same case. 24th: Sixth feed on another benign tertian case (taking quinine). 25th: Seventh feed on another benign tertian case (taking quinine). 26th: Eighth feed on same case as August 24. 27th: Dissected—all *nil*. Temperature of experiment: Average maximum, 29·8° C.; average minimum, 21·5° C.; absolute maximum, 32·9° C.; absolute minimum, 19·2° C.

(10) August 19: Two *A. superpictus* fed on benign tertian case (taking quinine.) 20th: Second feed on same case. 21st: Third feed on another benign tertian case (not taking quinine). 22nd: Fourth feed on same case. 24th: Fifth feed on another benign tertian case (taking quinine). 25th: Sixth feed on another benign tertian case (taking quinine). 26th: Seventh feed on same case as August 24. 28th: Dissected—both *nil*. Temperature of experiment: Average maximum, 29·8° C.; average minimum, 21·5° C.; absolute maximum, 32·9° C.; absolute minimum, 19·2° C.

(11) July 13: One *A. superpictus* fed on benign tertian case (taking quinine). 15th: Second feed on another benign tertian case (taking quinine). 16th: Third feed on another benign tertian case (not taking quinine). 18th: Fourth feed on another benign tertian case (taking quinine). 19th: Fifth feed on another benign tertian case (taking quinine). 20th: Dissected—stomach with three cysts in pre-sporozoite stage; salivary glands *nil*. Temperature of experiment: Average maximum, 34° C.; average minimum, 24° C.; absolute maximum, 35·5° C.; absolute minimum, 23·6° C.

(12) July 20: Three *A. superpictus* fed on benign tertian case (taking quinine). 21st: Second feed on same case. 22nd: Third feed on another benign tertian case (taking quinine). 23rd: Fourth feed on another benign case (not taking quinine). 25th: Fifth feed on another benign tertian case (taking quinine). 26th: Sixth feed on another benign tertian case (taking quinine). 27th: Seventh feed on another benign tertian case (taking quinine). 28th: Dissected—two *nil*, one with large number of pre-sporozoite cysts in stomach; salivary glands *nil*. Temperature of experiment: Average maximum, 33° C.; average minimum, 24° C.; absolute maximum, 35·3° C.; absolute minimum, 22·8° C.

(13) July 22: Two *A. superpictus* fed on benign tertian case (taking quinine). 23rd: Second feed on another benign tertian case (not taking quinine). 25th: Third feed on another benign tertian case (taking quinine). 26th: Fourth feed on another benign tertian case (taking quinine). 27th: Fifth feed on another benign tertian case (taking quinine). 28th: Dissected—one *nil*, one with good number of pre-sporozoite cysts in stomach; salivary glands *nil*. Temperature of experiment: Average maximum, 33·2° C.; average minimum, 23·4° C.; absolute maximum, 35·3°; absolute minimum, 22·8° C.

(14) September 13: Four *A. superpictus* fed on benign tertian case (taking quinine). 15th: Second feed on another case (taking quinine).

19th: Third feed on another case (not taking quinine). 23rd: Dissected—two *nil*, two with pre-sporozoite cysts in stomach; salivary glands *nil*. Temperature of experiment: Average maximum, 30·2° C.; average minimum, 21·3° C.; absolute maximum, 34° C.; absolute minimum, 20·5° C.

In these experiments of five *A. maculipennis* fed three became infected; while of fifteen *A. superpictus* five were infected. The mosquitoes were fed at least once on a case not taking quinine.

Experiments with *Plasmodium falciparum*.

All the cases employed for feeding were taking quinine.

(15) July 27, 1918: Two *A. maculipennis* fed on malignant tertian case. 31st: Second feed on same case. August 2: Dissected—*nil*. Temperature of experiments: Average maximum, 28° C.; average minimum, 22·1° C.; absolute maximum, 35·3° C.; absolute minimum, 24° C.

(16) September 9: Five *A. maculipennis* (hatched in laboratory) fed on malignant tertian case. 10th: Second feed on same case. 11th: Third feed on same case. 22nd: Dissected—four with six to ten cysts on stomach, of two sizes, some ruptured cysts; salivary glands heavily infected with sporozoites; one with cysts only and one black spore but salivary glands *nil*. Temperature of experiment: Average maximum, 30·5° C.; average minimum, 21·7° C.; absolute maximum, 33·9° C.; absolute minimum, 20·5° C.

(17) September 9: Three *A. superpictus* (hatched in laboratory) fed on malignant tertian case. 10th: Second feed on same case. 11th: Third feed on same case. 22nd: Dissected—one *nil*, two with two-sized cysts on stomach, ruptured cysts, salivary glands heavily infected with sporozoites. Temperature of experiment same as experiment 16. This experiment and No. 16 were carried out under the same conditions.

(18) October 4: Four *A. superpictus* fed on malignant tertian case. 5th: Four *A. superpictus* fed on malignant tertian case. 6th: Four *A. superpictus* fed on malignant tertian case. 7th: Four *A. superpictus* fed on malignant tertian case. 8th: Four *A. superpictus* fed on malignant tertian case. 9th: Dissected—*nil*. Temperature of experiment: Average maximum, 23·2° C.; average minimum, 17·2° C.; absolute maximum, 27·5° C.; absolute minimum, 15·5° C.

In these experiments seven *A. maculipennis* gave five positive results and seven *A. superpictus* gave two positive results.

Effect of Cold after Partial Development: First Experiment.

(19) October 16, 1918: Two batches of *A. maculipennis* and *A. superpictus* placed one on each of two crescent cases, A and B. The majority of the mosquitoes were known to have fed. 17th: Each batch placed on its case a second time. Placed in incubator at 21°—27° C. for eight days. 25th: Dissected five *A. superpictus* (Case A)—two with small cysts, salivary glands *nil*; dissected three *A. superpictus* (Case B)—three with

small cysts, salivary glands *nil*. Remainder placed in ice chest at 9°—12° C. November 2: Two *A. superpictus* (Case B) dissected—one *nil*, one with cysts same size as on October 25, salivary glands *nil*. Remainder divided into two lots:—

Returned to ice-chest 9°—12° C.

November 13: Two *A. superpictus* dissected—one with degenerate cysts; salivary glands *nil;* one *nil*.

Removed from ice-chest and incubated at 21°—22° C.

November 19: Dissected three *A. superpictus* and two *A. maculipennis* (Case B)—all with degenerate cysts in stomach; salivary glands *nil*. Dissected four *A. superpictus* and one *A. maculipennis* (Case A)—all with degenerate cysts in stomach; salivary glands *nil*.

Placed in incubator 21°—22° C.

November 5: One *A. superpictus* dissected—degenerate and crinkled cysts; salivary glands *nil*.

November 12: One *A. maculipennis* dissected—many degenerate cysts; salivary glands *nil*.

In this experiment of four *A. maculipennis* all were infected, while of twenty *A. superpictus* fifteen were infected. The exposure to cold had caused a degeneration in every case, with the possible exception of one (November 2).

(*To be continued.*)

PHYSICAL EXERTION, FITNESS AND BREATHING.[1]

By HENRY BRIGGS, D.Sc.,

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THE experiments discussed in this paper were carried out during the research on mine rescue apparatus which was instituted in 1917 by the Scientific and Industrial Research Department.[2] Their aim at first was limited to that of determining the oxygen consumption of persons engaging n different kinds of physical work, and with that object in view, a few tests were made by Dr. J. S. Haldane and the writer on miners climbing inclines in the Newbattle and Lingerwood collieries, Midlothian; these were shortly afterwards supplemented by other tests on the same men in which weights were lifted and certain of the common tasks of the miner were performed. During the early trials the men breathed ordinary air; but as the wearer of a mine rescue apparatus has to breathe air highly enriched with oxygen it was judged necessary to study the influence of such air on a person's capacity for physical work. It had been a matter of experience to the members of the Research Committee that they could perform work with greater ease and comfort while wearing a rescue apparatus in good order, and thus obtaining air containing seventy or eighty per cent of oxygen, than under normal conditions. The writer, for example, can climb a mountain faster and with less fatigue when using an efficient mine rescue apparatus than without it, notwithstanding that the apparatus weighs, in its latest form, about thirty pounds. It was observed that an increased oxygen proportion in the air inhaled was uniformly helpful with persons of sedentary habits, but that when working miners were tested little or no such benefit was derived; they were in fact generally quite indifferent as to whether they breathed air or oxygen.

A long series of experiments was then commenced in which Martin's ergometer was principally used as the means of measuring the rate of exertion, and in which a quantitative and qualitative examination was made with subjects of various physique and training breathing air and oxygen. Thanks to the kindness of the Superintendents of Physical and Bayonet Training, Scottish Command and Aldershot, several soldiers specializing in different branch of athletics were included in the tests.

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[1] Reprinted from the *Journal of Physiology*, vol. liv, No. 4, 1920.

[2] The Research Committee consists of Mr. William Walker, C.B.E., H.M. Chief Inspector of Mines (Chairman), Dr. J. S. Haldane, F.R.S., and the writer. Summaries of these experiments have been published in the writer's paper on "Fitness and Breathing during Exertion," *Journal of Physiology*, 53, *Proc. Physiol. Soc.*, p. 38, 1919, and in the *Second Report* of the Mine Rescue Apparatus Research Committee, 1920.

Subsequently, when it had become clear that the fitness of a subject could be measured by contrasting his respiratory performance when breathing normal air, and when breathing enriched air, the Army Council, acting upon the recommendation of Colonel Sir William Horrocks, K.C.M.G., C.B., and Lieutenant-Colonel E. P. Cathcart, of the Army Medical Department, set up a physical test station at which, up to the Armistice, the new method was applied for the examination of men sent in from units under the Scottish Command.

Apparatus and Methods.

The ergometer experiments were carried out in the Heriot-Watt College, Edinburgh, with *Martin's ergometer* [1]. During the experiments a pendulum, hanging in front of the subject as he sat in the saddle, provided the means of timing the rate of revolution of the pedals.[1] A rate of fifty-six revolutions per minute was adopted throughout. At that speed, and with the gear-ratio of the particular cycle employed, the power expended, in foot-pounds per minute, was ascertained by multiplying the difference of the balance-readings, in pounds, by one thousand.

Meters.—Two Milne dry meters were used. In each of them one revolution of the eight-inch pointer indicated the passage of one cubic foot of gas. The dials were marked off in hundredths of a cubic foot. The meters were tested against displacement from time to time. The barometric pressure, temperature and hygrometric state of gas being metered were kept under observation.

Douglas Bag and Sampling Apparatus.—A sixty-litre wedge-shaped bag was used to collect expired air. The bag, which is part of the Douglas respiration apparatus [2], is provided with a three-way aluminium stopcock which allows of the expired air being either discharged direct to the atmosphere ("off" position) or into the bag ("on" position). A small rubber tube connected to the bag enables samples of the contents to be drawn off for analysis. At first glass sampling tubes with taps at each end were used for this purpose, but they were soon abandoned in favour of small well-stoppered bottles, filled over a mercury trough. In the large numbers required the bottles were handier, simpler to use, less costly and much more easily replaced in case of breakage. A further simplification in taking samples for analysis was introduced at the Test Station.

Oxygen Cylinders and Reservoir Bag.—When the subject was breathing oxygen the gas was supplied from a 100-feet cylinder fitted with a reducing valve. The oxygen discharged from the valve into a reservoir bag. At first the subjects complained of parching of the throat when breathing oxygen; the trouble was removed by causing the gas to bubble through water before entering the reservoir. The latter was kept about three

[1] For long-continued pedalling the pendulum is preferable to the metronome for this purpose; it is less trying to the nerves.

parts distended during a "run." Over-distension was carefully avoided, as with excess of pressure in the reservoir bag it became possible for the gas to push open both breathing valves and to discharge direct into the Douglas bag.

Mouthpiece, Valves and Tubes.—The subject used a rescue-apparatus mouthpiece of rubber which fitted over one limb of a metal T-piece. The nose was closed by a clip. The air was directed to and from the mouthpiece by inspiratory and expiratory valves. Various valves were tried, the most successful being the Mueller water valve (fig. 1) and the Rosling valve (fig. 2). A Mueller valve of the dimensions indicated in fig. 1 allows of the heavy breathing of severe exertion without introducing a degree of resistance appreciable to most persons.

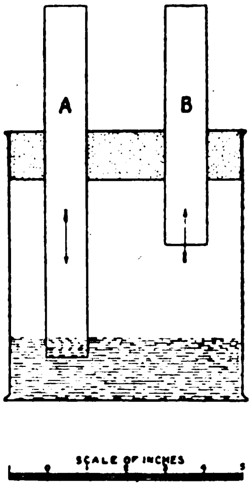


Fig. 1.—Mueller Water Valve.

Fig. 2.—Rosling Valve, shut and open.

The Rosling valve, adopted towards the end of the war for army anti-gas purposes, is very free from resistance and low in slip. While Mueller's is only serviceable for a stationary subject in the laboratory, Rosling's is equally useful in the laboratory and in testing men marching or climbing in the open or in the mine. Unlike the mica disk valve so frequently used in respiration experiments, it functions properly in any position. The valve is of rubber. A thin square of rubber, held by the corners, closes upon the flanged end of the tubular part of the valve (fig. 2, left-hand view) and flexes away from it when allowing air to pass through (fig. 2, right-hand view). The valve here illustrated was made for the writer by the Isleworth Rubber Company and adopted in the Briggs Mine Rescue Apparatus. Its resistance to a flow of eighty-five litres (three cubic feet) of air per minute is 0·35 inch of water column, and its slip is quite negligible. A not unimportant feature of the rubber valve is its noiseless-

ness. When a valve makes a distinctive noise the subject's attention is apt to be directed to his own breathing, and the test may be vitiated thereby. The tubes leading to and from the mouthpiece were of one inch bore—a size sufficient to reduce their resistance to negligible magnitude even with hard panting.

Sampling Tube for Alveolar Air.—A number of samples of alveolar air were taken while certain of the subjects were pedalling the ergometer. Essentially, the apparatus used for this purpose was that described by Haldane and Priestley [3]; it consists of a long tube through which the subject can empty his lungs suddenly. To avoid the subject having to close the end of the tube with his tongue after such an expiration, as was done in Haldane's and Priestley's experiments, a wide-bore tap was fitted at that end; on this being closed the man could replace the rubber mouthpiece (which for the moment he had withdrawn) and continue working, leaving the experimenter to draw out from behind the tap a small sample of air for analysis. With the quickened breathing incident to physical work, it was not possible to get alveolar air samples after inspiration and after expiration as may be done under rest conditions. Most of the experiments were on men altogether strange to scientific methods; few were sufficiently trustworthy when it came to the difficult operation of providing a reliable alveolar sample while doing hard work.

Gas-analysis Apparatus.—In the main set of experiments, in which both oxygen and carbon dioxide were determined in each sample of expired air, the Haldane gas-analysis apparatus [4] was used. At the test station, where the routine was simplified and only CO_2 ascertained, the Briggs' apparatus [5] was adopted.

Manner of Conducting Experiments.—A number of preliminary trials were carried out in order to settle such matters as the rest period needed between spells of work on the ergometer, and the length of time work must continue before the breathing becomes sufficiently *en rapport* with the exertion to permit of reliable observations being made. After that, a regular routine, based on Douglas' method, soon evolved, and the few changes subsequently made were merely to simplify the apparatus and connections and to improve the valves. This routine was as follows: The subject, seated at rest on the saddle of the ergometer and fitted with the noseclip and mouthpiece attachment, inhaled air from the room, drawing it through one of the Milne meters. The Douglas bag (now empty) was connected to the exhalation tube with the three-way cock in the "off" position to allow the products of respiration to escape into the room. When he had become accustomed to his position the cock was turned "on" at the end of an inspiration and the expired air began to enter the bag. A stop watch was started at the moment of turning the cock, and the number of inspirations was counted by watching the movement of the pointer of the meter. After about two minutes had elapsed, and again at the end of an inspiration, the tap was turned "off," and the watch

stopped. The bag was kneaded and one or more samples drawn from it. The volume in the bag was then measured by emptying the bag through the second meter. The necessary thermometer and hygrometer readings were taken. From these measurements and the time interval the volume exhaled per minute was calculated. The meter on the inspiration side enabled the quantity drawn into the lungs to be evaluated direct. Owing to the jerky action of the latter meter this determination did not reach the same degree of accuracy as that of the volume of exhaled air from the Douglas bag, but it was useful as a safeguard against gross error.

The second set of readings and samples were taken when the subject was pedalling with the belt off, i.e., when he was doing no external work. The same routine was followed, the man being required to pedal at the rate of fifty-six revolutions per minute for at least two minutes before commencing to collect the expired air. After this, similar records were obtained with gradually increasing loads. Longer rest intervals were allowed between the spells of work as the loads increased. The preliminary interval of two minutes' pedalling was strictly observed, except for the highest loads, such as 12,000 or 14,000 feet pounds per minute, which are beyond the capacity of even the strongest men to sustain for long. With excessive loads the preliminary interval had perforce to be shortened, though it was never allowed to be under one minute. The reduction of that interval, however, makes the determination of oxygen-consumption, etc., on the highest loads less reliable than those on more moderate rates of work, a feature which receives further consideration below.

After making a series of measurements with the man breathing air, an exactly similar series was made when breathing oxygen. Before commencing the latter the subject was required to breathe oxygen for at least ten minutes to expel the greater part of the nitrogen dissolved in the blood. The interval between the air and oxygen tests was usually some hours; on several occasions the two series were carried out on different days. In the cases of several of those tested at the college, and of nearly all those tried at the test station, no information was given to the subject as to whether he was breathing oxygen or air. It was thought best not to give a loophole for the prejudice, which is still curiously strong, against breathing oxygen for a few hours.

After a few tests with a simple pneumograph it was abandoned. As has been stated, the rate of breathing was ascertained by watching the pointer of the meter on the inspiration side, and as the meter had its back to the subject, he was generally unaware that any notice was being taken of his breathing.

Ergometer Tests.

Figs. 3 to 14 (see end of this paper) are typical graphed records of subjects undertaking work on the ergometer in the manner described. The small circles on the charts indicate values obtained when the men

were breathing normal air and the crosses those obtained when breathing oxygen; the more-or-less smoothed "air" curves are drawn in full lines and the "oxygen" curves in dotted lines. The output of CO_2 and the oxygen consumption are set out in litres per minute of dry gas at N.T.P. The volume ventilating the lungs is given in litres of saturated air or oxygen at blood temperature and normal pressure, expired per minute. The oxygen consumed was computed from the respiratory quotient and the CO_2 output.

The determination of the respiratory quotient by gas-analysis depends on the assumption that the mass of nitrogen inhaled is the same as the mass of nitrogen exhaled. The nitrogen, in fact, serves here as a measure or standard against which variations in oxygen are gauged. Evidently such a process will be more accurate when the nitrogen, as in ordinary air, exceeds the oxygen in volume: i.e. when the smaller is gauged by the greater. It will be less accurate when the nitrogen proportion is much lower than that of oxygen, as when cylinder oxygen is breathed, for then the greater is gauged by the lesser. As might therefore be expected, oxygen consumptions, calculated in the manner indicated, from measurements made when breathing cylinder oxygen, had a relatively high probable error. Another method of evaluating oxygen consumption was, however, applicable, owing to the volumes inhaled and exhaled being separately measured; and in case of doubt this second method was used as a check. It consists of finding (*a*) the volume of oxygen entering the lungs per minute (from the volume of enriched air inhaled and the proportion of oxygen in that inhaled gas); (*b*) the volume of oxygen leaving the lungs per minute (from the volume expired per minute and the proportion of oxygen in the expired air), when the difference (*a*)—(*b*) gives the required result.

Towards the end of the main series of experiments it was found that, even in a subject of low fitness, no advantage was to be secured by increasing the percentage of oxygen above sixty. Had that fact been known earlier, the work would have been facilitated by using a mixture containing sixty per cent oxygen and forty per cent nitrogen in place of cylinder oxygen; the higher proportion of nitrogen which would then have been available would have reduced the probable error of the oxygen consumption determinations on enriched air. At the physical test station sixty-seven per cent of oxygen was used instead of cylinder oxygen.

It has sometimes been advanced as a drawback to the Douglas method that the sample of exhaled air collected in the bag is contained over too short a period. For work of the character now being considered, however, the criticism would not appear to have much weight. If elementary precautions are taken, such as that of opening and closing the bag at the same stage in the breathing (e.g. at the end of inspiration), the shortness of the period of collecting the expired air is not a matter of consequence; of much greater importance is the length of the preliminary period during

which the man is required to work before the expired air is allowed to enter the bag. This should be uniform and adequate.

It was not practicable to put the subjects on a definite dietary. While this increased the degree of uncertainty of any single pair of observations, it does not affect the general results, since a large number of men were tested, and whenever doubt was felt in regard to the reliability of a set of measurements, the test was repeated on another day.

Normal and Overload.—Every-day experience proves that a muscular performance is easier when one is in "good condition." Equally commonplace are the facts that no task involving external work, not even the lightest, can be continued indefinitely without pause, and that the heavier the work the shorter the time it can be sustained. There are, however, certain lesser degrees of exertion (for instance walking or cycling at a moderate pace on a flat road), which, by the ease with which they can be kept up for hours on end, may be referred to, in electrical engineers' phraseology as "normal loads"; while other and heavier tasks (e.g. hard bayonet exercise or running quickly upstairs) are bearable for a limited period only and may be termed "overloads." What may be an overload to one person is a normal load to another who is stronger, or who is in better training or more habituated to the particular kind of labour. Again, a normal load when the person is fit may prove to be an overload when he is unfit; and, as has been remarked, even a light normal load if long supported without rest will eventually become an overload. Evidently, then, the whereabouts of the line demarcating between a normal and an overload for any individual depends on his condition at the time; if he is getting tired, it is moving down the scale of exertion; if resting it is moving up.

Oxygen Supply and Carbon Dioxide Output during Work.—An important difference between what we here term the normal load and the overload lies in their effect on the respiration after stopping the exertion. When one ceases an easy normal load like walking at three miles per hour along a flat road, the breathing quickly adjusts itself to the resting state: the after effect in a healthy person is nil. The influence of a severe overload is in marked contrast to this; when the work is stopped heavy breathing continues; the lung-ventilation falls to normal only after a period, which, in the case of a hard spell of work, may be many hours. In the first instance the oxygen intake was adequate; in the second it was not. Essentially, then, a normal load may be defined as one during the performance of which the oxygen supply is sufficient, and an overload one during which it is insufficient to satisfy in full the demands of the working muscles.

It is obvious that the supply of oxygen to the tissues may be deficient either in consequence of insufficient absorption in the lungs or inadequate circulation. Instances in which distress is produced by the rate of oxygenation of the blood failing to keep pace with the muscular demands,

though the circulation may be sufficient, are of great practical interest. They include the case of the poison-gas patient, where exudation and thickening of the epithelial layer of the lungs makes oxygen-penetration difficult; the case of the high-flying airman, where the low partial pressure of oxygen prevents proper oxygenation of the blood, and that of the so-called D.A.H. patient, where the shallowness of the breathing impairs the transfer of oxygen to the blood by insufficient exposure of epithelial area to freshly indrawn air [6].

A glance at the accompanying charts will show that when hard muscular work is being done the consumption of oxygen may rise to more than ten times the resting value. In the muscles at work there must be a much greater proportional increase of consumption, and such an increase can only be secured by an enormous addition to the blood circulation through those muscles. Failure to supply the additional blood, whether due to defects in blood-distribution or to cardiac efficiency, must, therefore, bring about local anoxæmia in the muscles, resulting in a cessation or reduction of the exertion.

Now it is known that, when muscles are insufficiently supplied with oxygen, lactic acid is formed; indeed that when an extreme overload is attempted, such as running quickly up several flights of stairs, the blood is at once flooded with lactic acid. The highly stimulative influence of lactic acid upon the respiratory centre and the relative slow rate at which it disappears from the blood are also well known. The formation of this acid would therefore appear sufficient to account for the falling off of the percentage of CO_2 in the expired air which (as the curves show) is the invariable rule when the load is increased beyond a certain amount, and would also partly explain the long continued enhanced breathing after the cessation of a heavy overload.

Since the appearance of lactic acid in the blood is a sure sign of overload, and since that appearance is characterized by a fall in the proportion of CO_2 expired, the writer feels justified in taking the rate of work corresponding to the maximal CO_2 proportion in the expired air as the boundary between an overload and a normal load, while breathing air or oxygen as the case may be. This boundary, it will be understood, can only be a rough one. Nor is it a stable one; fatigue moves it down the scale. Again, the fact that in most cases (e.g. fig. 4) the expired-CO_2-percentage curve gradually flattens as the crest of the dome is approached, appears to denote the onset of oxygen-want before the "crest-load" is reached; but since that influence (except in the immediate region of the crest load) is not usually serious, the subject can support such rates of exertion for a considerable time. In other words, though there is in the charts good evidence that the call for oxygen by the working muscles becomes (either *per se* or through the agency of lactic acid) a partner in respiratory control even on relatively light exertion, the demand appears to be satisfactorily met until the rate of work is increased up to, or nearly up to, what is here termed the "crest load."

Stamina.—Stamina is taken to mean the power of supporting continuous exertion. It will be apparent that the higher the "crest load" the larger will be the range of loads which can be dealt with without oxygen-want bringing the exercise to an end. A given rate of work may be a normal load to one man whose "crest load" is high and an overload to another whose "crest load" is low. Thus the crest load (the abscissal position of the crest of the dome of the exhaled-CO_2-percentage curve) becomes a measure of the stamina for the particular kind of work in question.

In every case but one (Subject VIII) the crest load was higher (i.e., the crest was further to the right) when breathing oxygen than when breathing air. In most cases, that is to say, the boundary between normal and overload moves up the scale, and the subject's capacity for sustained exertion is improved, as the partial pressure of oxygen in the inhaled air, and, therefore, in the alveolar air, is increased. The lower the person's fitness the greater the improvement brought about.

Alveolar CO_2 during the Accelerative Period.—As has already been stated, a rate of work like 12,000 foot-pounds per minute, was too heavy to be kept up long by any of the men tested, and on such loads it was not possible to wait the usual two minutes before taking the samples and readings. The result was that on the heaviest loads, the latter were taken during the accelerative period. In some instances (Subjects II, III, IX, XIII, XV), alveolar samples were obtained during that period, and the CO_2 percentages are shown on the graphs. They will be seen to be unusually high; indeed with Subject XIII, breathing air, the record figure of 10·1 per cent is reached. The matter lay outside the scope of the research and was not pursued further; but it would seem questionable whether these high CO_2 tensions are possible in the alveolar air without active excretion of CO_2 on the part of the lung-epithelium.

Fitness and Expired-CO_2 Percentage.—The graphs may now be examined with a view to ascertaining the influence of fitness on respiratory behaviour.

The high level of the CO_2 percentage in the air expired by most fit persons doing work is perhaps the first feature to attract attention. It is a usual but not an invariable attribute of the fit man that he can stand a higher CO_2 and a lower oxygen percentage in the alveolar air than the unfit man performing the same task; he makes more use of the air he inhales and therefore requires less of it. Thus, in contrasting the very striking athletic subject XIII (condition (A) fig. 10) with the sedentary Subject II (fig. 4), when both are breathing normal air, the maximal CO_2 percentage in the former case is seen to be 8·1 and in the latter case 4·7, and, although the heavier man, XIII can work the ergometer on a lung-ventilation of less than half that required by II. The highest CO_2 and lowest oxygen proportions were recorded in the case of those to whom slow, deep breathing is habitual during physical exercise. While working at the rate of 6,000 foot-pounds per minute, for

example, II breathed twenty-four times per minute, while two unusually deep breathers, Nos. XIII and VIII, respired eight times and twelve times per minute respectively on that load. That the correspondence of high fitness and a high CO_2 level is not invariable is shown by a subject (graphs not reproduced), a very fit young athlete, whose expired-CO_2-percentage reached a maximum of only 4·8 when breathing oxygen.

Fitness Measurement.—Perhaps the most interesting and useful of the results obtained follows from a comparison of the curves of exhaled-CO_2-percentage when the subject breathes air and when the subject breathes oxygen. In the case of a relatively unfit man, such as II, these curves diverge; but in that of VIII (fig. 8)—an Army instructor in physical drill selected for experiment by the Scottish Command as representing physically the best the Army can produce—the curves are almost coincident and their crests actually coincide. Observations on many subjects have warranted the conclusion that fitness is inversely as the degree of divergence of the two CO_2 curves. The most convenient manner of evaluating fitness proved to be the following: having drawn the two contrasting curves (work done, abscissæ; CO_2 percentages, ordinates) the expired-CO_2-percentage, with the subject at rest and breathing normal air, was marked by an arrow-head on the Y-axis of the graph. A horizontal line having then been struck across the chart through the arrow-head, the vertical distances between that line and the crests of the "air" and "oxygen" curves were measured off. The fitness factor was then taken to be the first of these distances divided by the second. By this method the fitness of Subject II was 46 per cent and that of VIII was 100 per cent. The factors for the other selected subjects are stated in the Appendix.

The assumption underlying this mode of expressing fitness is twofold: first, there is, as basis, the conception of zero fitness as being the state in which the CO_2 curve on air falls away from the Y-axis, or, in other words, in which the crest lies on that axis at a point coincident with the resting value of the CO_2 percentage. That is to say, zero fitness is regarded as the condition in which the slightest load is an overload and where oxygen want becomes serious when the least exertion is attempted. Secondly, there is the assumption that breathing oxygen raises fitness (as regards the lungs) to 100 per cent. The first point will be readily conceded; as to the second, the evidence appears conclusive. Subject III was tested on occasions several months apart; the first time he was in low health and his fitness factor was forty-four per cent; the second time he was well and the factor had risen to eighty per cent; but the CO_2 curve on oxygen was substantially the same in each case. Subject XIII was frequently tested over a period of six months. At first he was in normal health and had a fitness of seventy per cent. He was then sent to Aldershot for the final course of training for serjeant-instructors in physical drill and returned to Edinburgh in the "pink" of condition, for further test after being a

fortnight at Aldershot. It was then found that while the "oxygen" curve was substantially as before, the "air" curve had risen to meet it, and that, indeed, the two curves agreed up to the crest. In other words, fitness had become 100 per cent. Some time after, XIII was transferred back to Scotland under medical orders; he had become very "stale" and run down. He was again tested and found to have a fitness of fifty-five per cent; but, as before, the change was evidenced by a movement of the "air" curve only.

It is to be observed from the results that, when an overload is being dealt with, even the fittest men derive some assistance from breathing enriched air, while the unfit benefit to a still greater extent. An overload to a relatively unfit person breathing ordinary air may become a normal load when he breathes, say seventy per cent oxygen. A man getting fatigued while supporting what was at first a normal load but which has now become an overload, no matter how fit he may be, is relieved by breathing enriched air—an effect which has been remarked by other observers. Conversely, heavy work can be accomplished with less fatigue when respiring oxygenated air *continuously* from the commencement.

The method of measuring fitness described above involves the assumption that lung-fitness indicates general physical fitness. Such appears actually to be the case if an exception be allowed in the instance of persons inured to living at a high altitude; in those circumstances the required degree of adaptation is not derived so much from physical exercise as from long-continued exposure to low oxygen pressure, and the lungs may be highly efficient without general bodily fitness being a necessary consequence.

Bearing on the Oxygen Secretion Question.—Since an unfit man derives much benefit during muscular exertion through addition of oxygen to the inspired air, while a fit man is very little benefited, it seems clear that the lungs of the fit man absorb oxygen more readily from normal alveolar air during exertion. This might be due either to some anatomical change which makes simple diffusion occur more readily through the lung epithelium of the fit man, or to active secretion of oxygen inwards by the lung epithelium.

The former theory does not seem inherently probable; but if it were correct we might expect that even during rest the alveolar CO_2 percentage would be higher among fit than among unfit men. To ascertain whether this is so the records of eighty-four men were examined. They were of every medical category, though the "A" class predominated. Their ages ranged from 15 to 50, though most were of the usual military age. The following table sets forth the expired CO_2 percentage sitting at rest against the fitness factor, the latter having been determined as described above.

The evidence is emphatically negative; the expired-CO_2-percentage

at rest, and therefore, by inference the oxygen tension of the alveolar air at rest, is not affected by a very large variation in fitness.

TABLE I.

Fitness per cent	Number of subjects examined	Average expired-CO_2-per cent at rest
40—50	6	3·52
50—60	10	3·75
60—70	23	3·66
70—80	19	3·61
80—90	20	3·52
90—100	6	3·60

The secretion theory as propounded by Bohr and by Haldane and his co-workers affords a more probable explanation. The theory predicates that the epithelial cells possess the power, which they exercise in response to stimuli originating in anoxæmia of the tissues, of secreting oxygen from the alveolar air into the blood [7]. When a person is at rest he gets oxygen by simple diffusion; but during work, or during existence at a high altitude, the amount so obtained is inadequate and is supplemented, as shown by the experimental data of these observers, by secretion. Once these cells are regarded, so to speak, as oxygen pumps which can be set going when required, the experimental results described above become intelligible. Practice or training facilitates the oxygenation of the blood by improving the cells' power of secretion. In the fittest men, no benefit is derived during normal load from breathing enriched air, since they are able to get from normal air by secretion all the oxygen they need. The existence, in the lung epithelium, of a capacity which can be developed and intensified by training or other means of adaptation and which inferentially may be impaired by overwork or overstrain, throws a new light on the phenomena of respiratory fatigue.

Oxygen Consumption.—Table II, which has been drawn up from the smoothed curves, gives, in litres per minute of dry gas at N.P.T., the oxygen consumption of the selected subjects while doing work on the ergometer and while breathing both normal air and oxygen.

Efficiency of Ergometer Work.—The curves relating to efficiency at different loads have a certain interest, though of all the results these are perhaps most open to criticism and require most qualification. They were computed from the oxygen consumptions and from Zuntz's table of energy-equivalents [8]. They are "gross" or "overall" efficiencies and give, at different loads, the relation between the useful external work done by the human machine and the energy generated within that machine by exothermic chemical changes. A person pedalling the ergometer with the belt off and thus doing no external work has, on this basis, zero efficiency. The accuracy of these estimates depends on several factors. The manner of arriving at Zuntz's figures has, the writer believes, been considerably criticized, though in view of the other sources of error now to be noted small imperfections in those values are barely worth considering. The

evaluation of efficiency on normal load is comparatively straightforward and probably fairly exact, but grave difficulties arise when overloads are being dealt with. Being determined from the oxygen consumption, an efficiency is evidently affected by error in measuring the oxygen consumed. Further, it is important to realize that, as a statement of the rate of oxygen consumption *at a given time*, say two minutes after starting an overload, a certain value may be accurate and yet it may yield altogether misleading results if used as the basis for calculating efficiency *at the same time*. This conclusion follows from the fact that to measure efficiency accurately there must be a correct correlation of energy-intake and energy-output. There is, however, no such agreement during an overload when the output of energy is, for a time, excessive. The portions of the curves which are considered unreliable for this reason are shown by even dots.

TABLE II.—OXYGEN CONSUMPTION. ERGOMETER EXPERIMENTS.

Subject	Work done in foot-pounds per minute							
	Sitting at rest		3,000		6,000		9,000	
	Breathing air	Breathing O_2	Breathing air	Breathing O_2	Breathing air	Breathing O_2	Breathing air	Breathing O_2
I	0·28	0·31	1·23	1·05	1·77	1·55	2·42	2·15
II	0·23	0·31	1·23	1·20	2·08	1·82	2·42	2·30
III	0·37	0·22	1·03	0·81	2·62	1·30	2·20	2·10
IV	0·33	0·28	1·12	0·95	1·68	1·61	2·04	2·30
V	0·28	0·37	1·17	0·81	1·80	1·44	—	—
VI	0·47	0·40	1·17	1·02	—	—	—	—
VII	0·45	0·40	1·13	1·05	1·70	1·49	2·37	1·90
VIII	0·42	0·32	1·05	0·97	1·58	1·50	2·27	2·00
IX	0·40	0·30	1·00	1·05	1·68	1·62	2·30	2·30
X	0·37	0·28	1·12	0·81	1·83	1·28	2·52	1·70
XII	0·40	0·41	1·07	0·92	1·58	1·54	2·09	2·02
XIII (*a*)	0·33	0·45	0·70	1·05	1·17	1·54	1·75	2·20
(*b*)	0·37		1·30		2·00		2·65	
XIV	0·38	0·25	1·17	0·92	2·09	1·65	2·62	2·52
XV	0·25	0·35	1·07	0·98	1·63	1·50	2·30	2·30
XVIII	0·42	0·24	1·30	1·00	2·00	1·67	2·71	2·31
Average	0·36	0·32	1·12	0·97	1·74	1·54	2·33	2·16

If efficiency had been the subject under study, it would have been necessary to endeavour to correct the curves by taking into account the excess oxygen consumption during the post-work period. An interesting feature (which has been noted by previous workers) becomes apparent when an efficiency curve is corrected in that manner; it is then seen to be dome-shaped: in other words, as the load increases the efficiency reaches a maximum and then falls away again. The maximum occurs at or near the line of demarcation of normal load and overload. Even with the uncorrected efficiencies graphed, the tendency towards the domed form

can be detected in several cases, as for example in that of Subject IX. Zuntz's values being based upon the respiratory quotient, the abnormality of the respiratory quotient during severe work is another disturbing factor.

Generally speaking, the efficiency appears to be greater when breathing oxygen than when breathing air. With the relatively unfit person that effect may be partly due to the fact that, for a given load, less energy is consumed in respiration when breathing oxygen.

The efficiency of the fit is greater than that of the unfit man. This again may to some extent be owing to the relatively small respiratory effort of the fit person: but no doubt the fact of the fit man being usually habituated to physical exertion, and having learnt to deal with a task with a minimum waste of muscular energy as possible, has a great deal to do with his higher efficiency. For example, the expenditure of energy and consumption of oxygen involved when a miner uses a shovel are markedly less than when the same task is performed by a person unaccustomed to shovelling.

Climbing and Walking Experiments.

A number of experiments were made on men climbing the main incline of the Burdiehouse limestone mine, Midlothian, both while breathing normal air and while breathing oxygen. Preliminary tests in the Lingerwood and Newbattle collieries had shown the advisability of limiting the variables. This could be done either by taking one subject on a number of gradients or by taking several subjects on one gradient, and the latter alternative was chosen as being likely to give most information. The Burdiehouse incline lies at a uniform slope of 21°. The roof is high, so that there was no occasion to stoop, and the floor, while dry for the most part, was, at the time of the tests, wet and slippery in places. On the whole the condition of the incline might be taken as a fair average of that of a mine roadway of heavy grade. Owing to the difficulties encountered in fitting up, each day, a temporary laboratory on the side of the roadway, I had to be satisfied with a few determinations for each man; usually values were obtained at five rates of speed, both when breathing air and when breathing oxygen.

It was intended to put the results, especially as to oxygen consumption, in the most useful form for designers and users of mine rescue apparatus; therefore the subject carried such an apparatus both during the climbing tests and during the walking and running trials on the flat which are referred to below. The total weight borne on each occasion was about forty-three pounds. The values thus apply to fully equipped infantrymen. The procedure during these experiments was the following:

Breathing Normal Air.—The subject carried a Douglas bag on his back and an exhalation bag, fitted with a relief valve, on his chest. He breathed through a mouthpiece, his nose being clipped. Inhalation and

exhalation valves were so placed that he drew air from the Douglas bag and expired into the exhalation bag. Before starting, the Douglas bag was inflated with a measured volume of air by aid of a large double-acting bellows. The man was then set to walk up the incline (which was marked off in chains and poles) at the desired rate. The three-way tap of the Douglas bag being "off," he inspired, at first, from the atmosphere. When it was judged that his respiration had adjusted itself to the degree of exertion, the three-way tap was turned "on" and he began to breathe from the measured volume in the Douglas bag. The length of the spell of work, from the moment of turning on the tap to the moment of turning it off again, was taken by a stop-watch. After the spell samples for analysis were withdrawn, over mercury, from the exhalation bag, and the volume remaining in the Douglas bag was metered.

Breathing Oxygen.—Before any observations were made on oxygen, the man was required to use the mine rescue apparatus which he was carrying for a sufficient time to remove the bulk of the free nitrogen dissolved in the blood and tissues. During this preliminary period the nitrogen percentage in the air of the closed circuit of the apparatus was kept low by frequently washing out through the relief valve with excess oxygen. After that operation the subject was not allowed to breathe ordinary air until the whole of the oxygen series of tests was completed. During rests, and during the first parts of a climb while the respiration was accelerating, he used the rescue apparatus. The routine was the same as that described above, the Douglas bag, however, being filled with a measured volume of oxygen, and the subject, on the word of command, changing rapidly from the rescue apparatus mouthpiece to that of the respiration apparatus, or vice versa.

The walking and running tests were made on a smooth, level concrete track at the mine rescue station, Edinburgh.

The results of two such tests are set forth in figs. 12 and 13. Fig. 14 is constructed from information relating to "C. G. D." and obtained from a paper by Haldane and Douglas [9]; it is included for the sake of comparison. In condition (*a*), indicated on the graph by full lines, this subject was breathing normal air and walking on the laboratory floor, while in condition (*b*), indicated by chain dots, he was breathing air and walking in a level grass field. He did not carry a load; therefore, the consumption of oxygen and production of carbon dioxide are relatively less than those of the other subjects. Though the figures actually obtained in Haldane and Douglas' experiments were used in drawing the graphs, twenty-five per cent has been added to "C. G. D.'s" oxygen consumptions in the following table to make them more comparable with those of the other men, who were carrying forty-three pounds each.

Tables III, IV and V, derived from the smooth curves, state oxygen consumptions (expressed in litres per minute of dry gas at N.T.P.) of men walking and running on the flat and climbing the Burdiehouse incline:

TABLE III.—OXYGEN CONSUMPTION.* WALKING AND RUNNING ON THE FLAT CARRYING WEIGHT OF 43 LB.

	Miles per hour											
	Standing		1		2		3		4		5	
Subject	Br. air	Br. O_2	Br. air	Br. O_2	Br. air	Br. O_2	Br. air	Br. O_2	Br. air	Br. O_2	Br. air	Br. O_2
I	0·71*	0·68*	0·77	0·91	0·85	1·14	0·95	1·40	2·12	2·22	3·00	2·80
II	0·47	0·53	0·59	0·59	0·80	0·74	1·15	1·06	1·68	1·59	2·20	2·10
III	0·25	0·41	0·64	0·47	0·90	0·70	1·17	0·96	1·62	1·78	2·30	2·80
XIII	0·31	0·28	0·60	0·63	0·88	1·00	1·20	1·38	1·60	1·75	2·90	2·70
XVI	0·55	0·57	0·70	0·84	0·92	1·22	1·31	1·65	2·09	2·11	3·40	2·70
Average	0·40	0·45	0·65	0·69	0·87	0·96	1·16	1·29	1·82	1·89	2·80	2·60
C.G.D.(*a*)	0·40	—	0·60†	—	0·84	—	1·14	—	1·47	—	2·65	—
(*b*)	0·40	—	0·67†	—	0·98	—	1·33	—	1·99	—	3·17	—

* Unusually high : omitted in averaging.
† Interpolated from the graph.

TABLE IV.—OXYGEN CONSUMPTION. CLIMBING MINE INCLINE OF 21°, CARRYING 43 LB.

	Work done in foot-pounds per minute							
	Standing		3,000		6,000		9,000	
Subject	Breathing air	Breathing oxygen	Breathing air	Breathing oxygen	Breathing air	Breathing oxygen	Breathing air	Breathing oxygen
I	0·62	0·57	1·08	1·37	2·02	2·17	3·10	3·00
II	0·42	0·40	1·14	1·19	2·20	2·55	—	3·40
III	0·35	0·41	1·14	1·09	2·19	2·05	—	—
XIII	0·47	0·45	1·40	1·24	2·56	2·37	3·10	2·60
XVI	0·45	0·47	1·24	1·24	2·02	2·02	2·80	2·85
Average	0·46	0·46	1·20	1·23	2·20	2·21	3·00	3·00

TABLE V.—OXYGEN CONSUMPTION. CLIMBING MINE INCLINE OF 21°, CARRYING 43 LB.

	Speed in miles per hour; slope measurement							
	Standing		0·5		1·0		1·5	
Subject	Breathing air	Breathing oxygen	Breathing air	Breathing oxygen	Breathing air	Breathing oxygen	Breathing air	Breathing oxygen
I	0·62	0·57	1·10	1·40	2·18	2·25	3·20	3·10
II	0·42	0·40	1·10	1·14	2·15	2·50	—	3·40
III	0·35	0·41	1·25	1·19	2·45	2·30	—	—
XIII	0·47	0·45	1·50	1·31	2·70	2·46	3·20	2·70
XVI	0·45	0·47	1·26	1·26	2·04	2·04	2·80	2·90
Average	0·46	0·46	1·24	1·26	2·30	2·31	3·10	3·00

Most economical rate of walking.—Like a steamboat or an airship, a man has a most economical speed at which he goes farthest per litre of oxygen or per pound of fuel or food consumed. The data obtained yielded the following information: "C. G. D.'s" most economical speed while breathing air and walking without burden on the laboratory floor, was four miles per hour, at which rate he moved ninety-nine yards per litre of oxygen consumed. Walking without burden on grass, the same subject's most economical speed was three miles per hour, when a litre carried him eighty-two yards. With all the other subjects of Table III—

loaded, as each of them was, with forty-three pounds—the most economical speed proved to be three miles per hour when breathing air, while, when breathing oxygen and similarly loaded, that rate was three miles per hour for I, II and III, and four miles per hour for XIII and XVI. It is apparent that increased difficulty of walking, whether due to the man carrying a weight or to lack of smoothness of the path, reduces the most economical speed.

The writer expresses his obligation and gratitude to Dr. J. S. Haldane for the encouraging interest he took in these experiments and for his equally invigorating criticism. Mention must also be made of the loyal assistance given by Miss Elizabeth Gilchrist, M.A.., B.Sc., and Mr. David Penman, B.Sc., in conducting the experiments; of the painstaking work of the Physical Test Station staff, and of the very willing help given by a great many mine officials, miners, soldiers, and others in the course of tests which were often of an arduous nature.

Summary.

(1) Physical work is found by experience to be easier to unfit men when oxygenated air is breathed than when normal air is breathed, but no such difference is to be observed with fit men.

(2) When exertion of steadily increasing magnitude is undertaken, the expired-CO_2-percentage first rises and then falls. The load at which that percentage is a maximum is called the "crest load." It is shown that the crest load demarcates between normal loads and overloads. The demarcation line is not constant, and the circumstances causing movement of that line are discussed.

(3) If curves be drawn showing work done (abscissæ) and exhaled-CO_2-percentage (ordinates), (*a*) when the subject breathes air, and (*b*) when he breathes oxygen, the curves are found to coincide up to the crest where the man is very fit and to diverge widely when he is unfit, since the CO_2-percentage becomes much lower in the unfit when only ordinary air is breathed. A method of measuring fitness is described; it is based upon the experimental fact that fitness is inversely as the divergence of these curves.

(4) On an overload, even the fittest man derives benefit from breathing enriched air.

(5) The nature of the adaptation produced by physical training and by certain vocations is compared with that found to result from living at a high altitude. The bearing of the results upon the oxygen secretion question is considered and reasons are given for the acceptance of the secretion hypothesis.

(6) The benefit of breathing enriched air when doing physical work is limited to air containing about 60 per cent oxygen. Enrichment above that proportion has no effect during exertion, even on very unfit persons.

(7) Tables are inserted setting forth the oxygen consumptions of

numerous subjects while working the ergometer, while walking and running on the flat, and while climbing a mine incline of 21° slope, and the most economical rates of walking are shown for several subjects.

Appendix. Description of Subjects selected for Illustration.

Subject I (fig. 3).—Miner (repairer) working in steep seams, weight, 154 pounds, fitness eighty-three per cent; the position of the "peak load" (7,500 to 8,000 feet pounds per minute) is higher than the average, indicating a man of good stamina.

Subject II (fig. 4).—Sedentary person; weight, 136 pounds; fitness forty-six per cent; finds work much easier when breathing oxygen; oxygen-want apparent at relatively low rates of exertion.

Subject III (fig. 5).—Instructor at a mine rescue station; weight, 165 pounds; fitness eighty per cent.

Subject IV (fig. 6).—Mine undermanager; weight, 168 pounds; engaged in a mine working flat seams; fitness, sixty-four per cent; oxygen-want becomes serious under 6,000 foot-pounds per minute.

Subject VI (fig. 7).—Army recruit; previously a bank clerk; weight, 142 pounds; fitness, forty-two per cent.

Subject VIII (fig. 8).—Regular soldier; weight, 168 pounds; instructor in physical drill; heavy weight lifter; athletic type; fitness, 100 per cent. Judging from his general behaviour while doing work (quite apart from the results obtained) he is the fittest man of the series. Breathing oxygen is not the least benefit until the crest load is exceeded; then it gives a gradually increasing assistance.

Subject IX (fig. 9).—Mine fireman or deputy, working in a flat-seam colliery; weight, 168 pounds; fitness, sixty-nine per cent.

Subject XIII (fig. 10).—First-class footballer; runner, jumper and all-round athlete; army instructor in physical drill; weight, 158 pounds. Records were obtained of the subject in three conditions: (1) In good health; fitness, seventy per cent (the ergometer results labelled (*A*) were obtained on that occasion); (2) in good health and after intensive training at Aldershot; fitness, 100 per cent; and (3) in poor health; fitness fifty-five per cent. The ergometer tests marked (*B*) and the climbing and walking tests were carried out with the man in the last condition. The low rate of breathing, small lung-ventilation, great depth of breathing and abnormally high CO_2-percentage level are remarkable features.

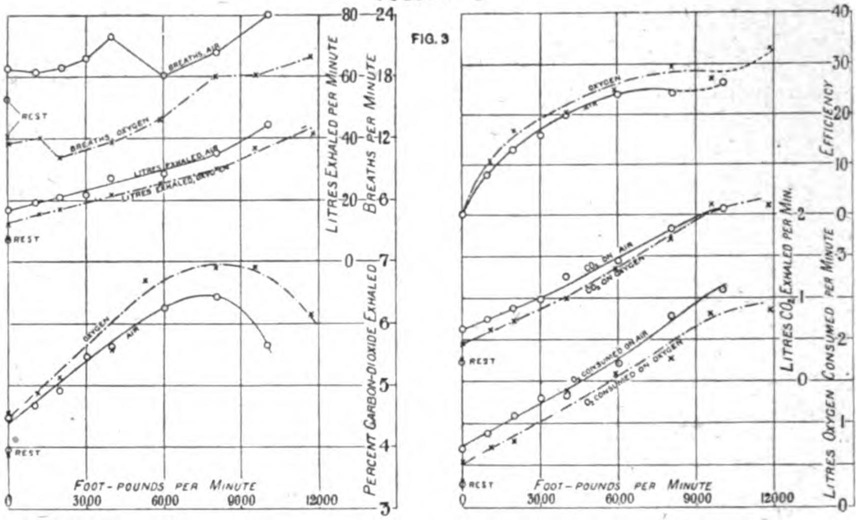
Subject XV (fig. 11).—Assistant instructor at a mine rescue station; weight, 147 pounds; fitness, sixty-three per cent.

Subject XVI (figs. 12, 13).—Research assistant, sedentary habits; weight, 154 pounds.

C. G. D., fig. 14.

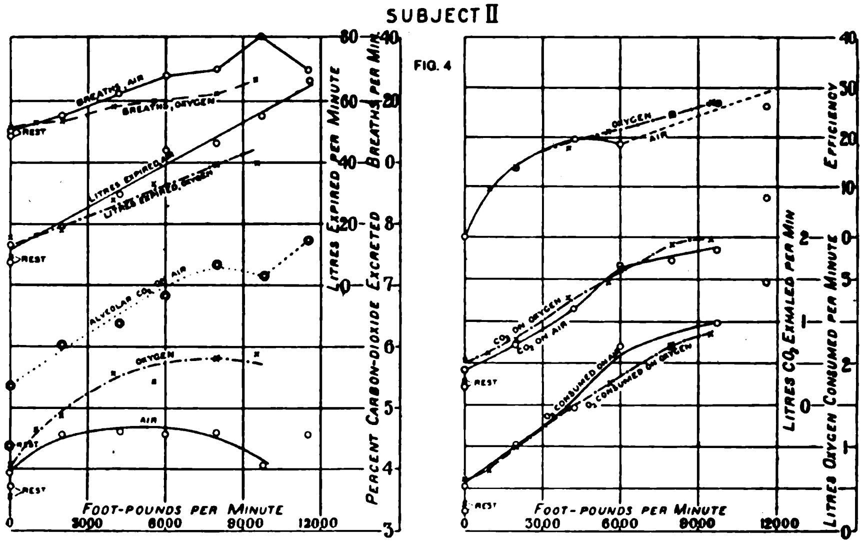
SUBJECT I

FIG. 3



SUBJECT II

FIG. 4



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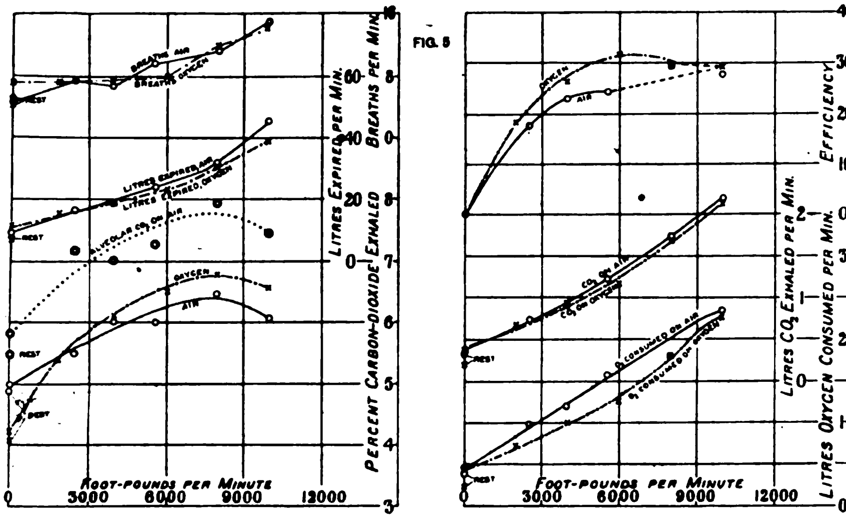

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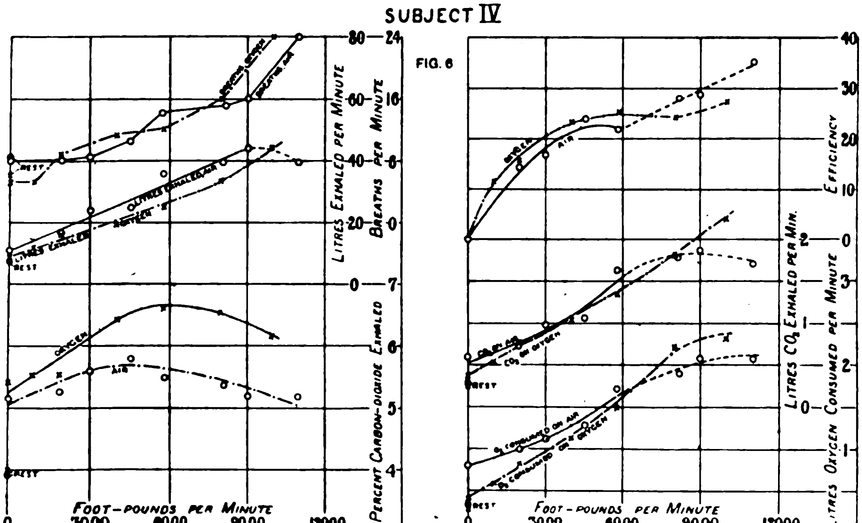

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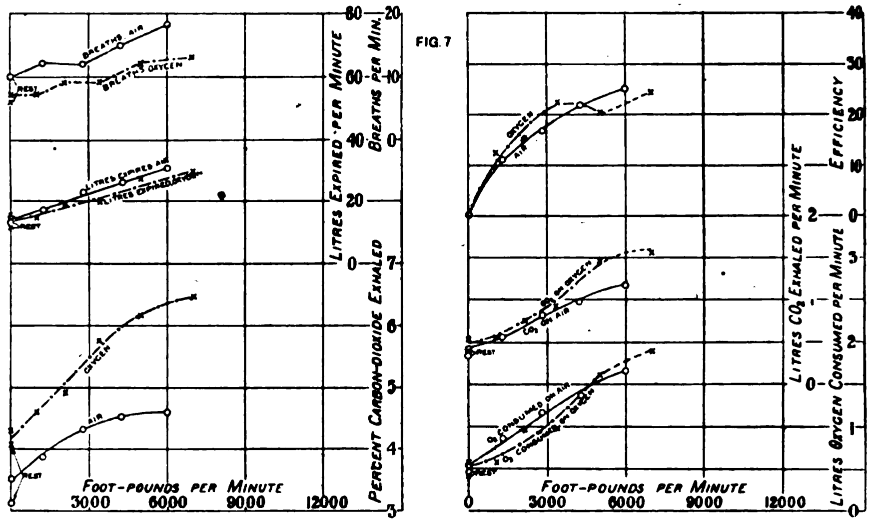

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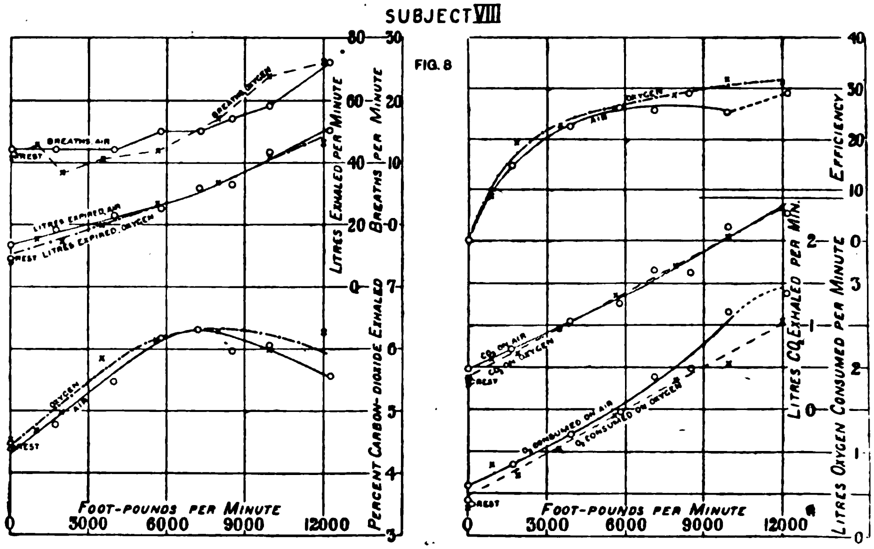

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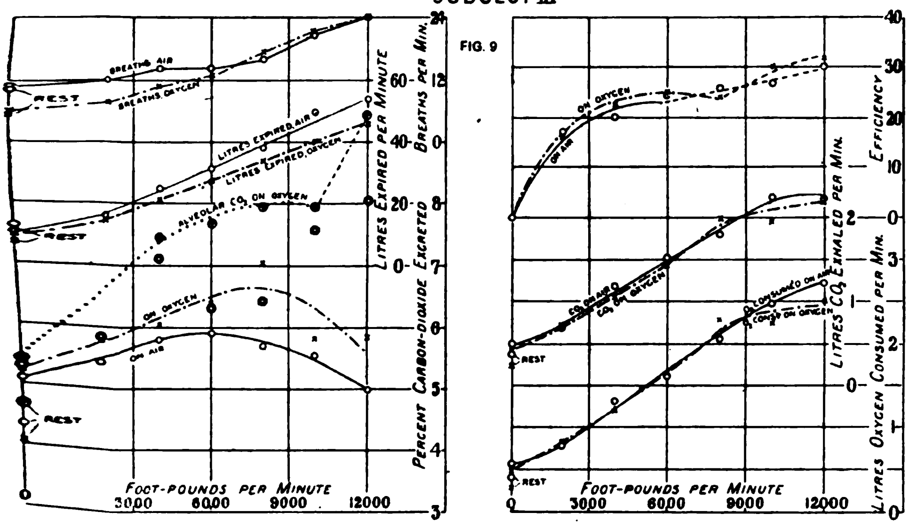


FIG. 9

SUBJECT XIII

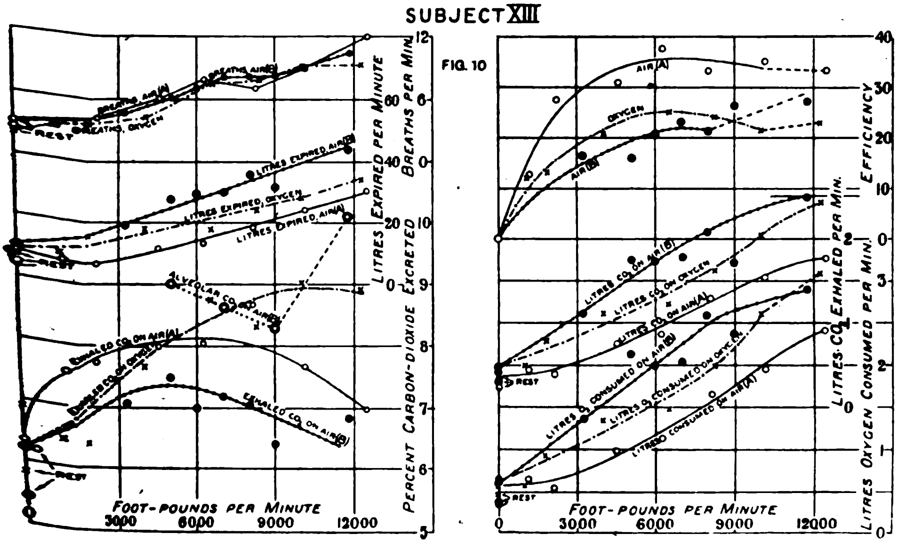
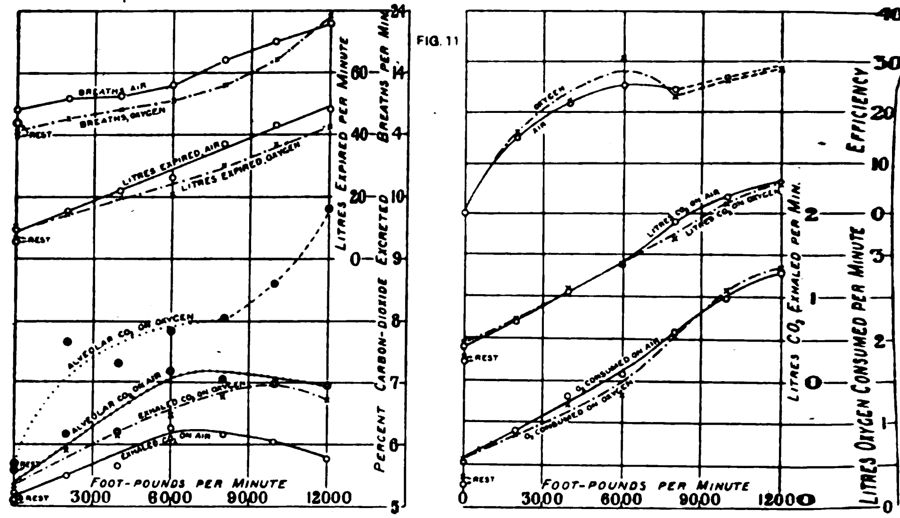


FIG. 10

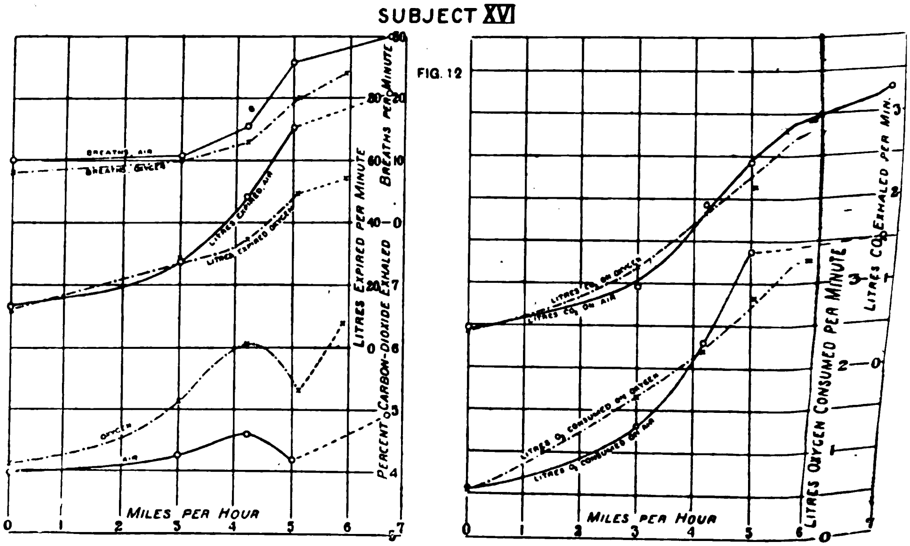
SUBJECT XV

FIG. 11

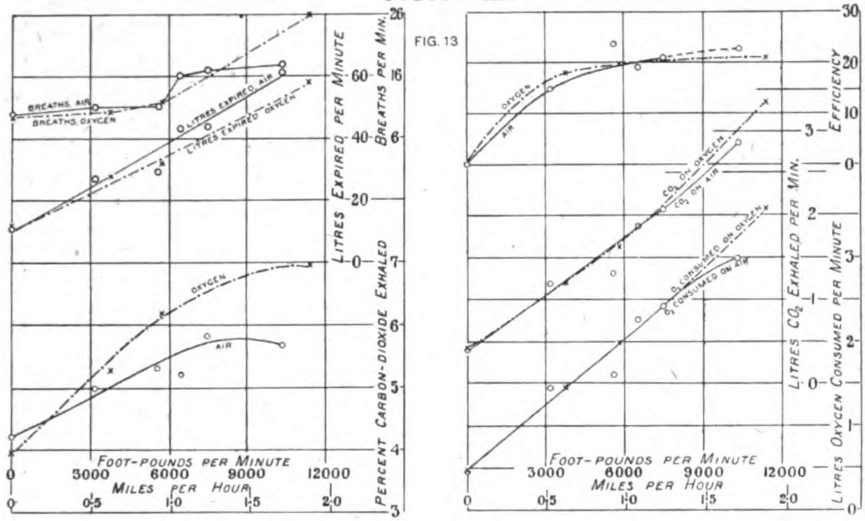


SUBJECT XVI

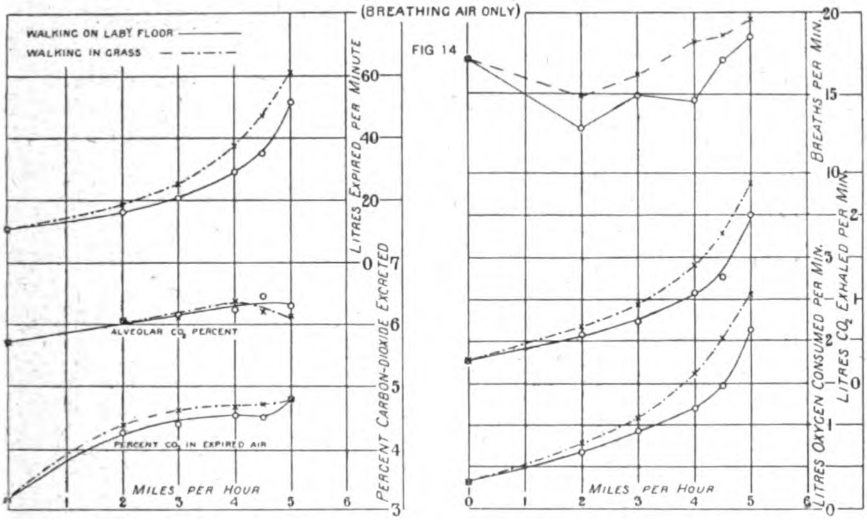
FIG. 12



SUBJECT XVI



SUBJECT–C.C.D.
(BREATHING AIR ONLY)



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Clinical and other Notes.

NOTES ON A CUTANEOUS REACTION FOR THE POSSIBLE DIAGNOSIS OF ENTERIC GROUP CARRIERS—ENTERIC AND DYSENTERY CONVALESCENT DEPOT, WELLINGTON.

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An attempt has been made by a series of experiments and records throughout the year to ascertain if any reliable cutaneous reaction is given by enteric group carriers, after the manner of a von Pirquet reaction.

The only reference in the literature available is one giving a series of reactions for testing the time limits of T.A.B. inoculations by Gay and Force in their typhoidin test—*British Medical Journal*, October, 1917. At the beginning of the year while discussing anaphylaxis in connexion with serum anaphylaxis cases which had occurred in the station and also general reactions to vaccines, it occurred to me that possibly enteric carriers might give either an enhanced reaction to dead cultures of the causal organism, or show local anaphylaxis to a serum possessing high amounts of antibodies to that causal organism.

Accordingly a series of small experiments was commenced with a view to test the local skin reactions for all enteric cases in the depot. There were at that time several cases being tested, one actual carrier, and also three officers who had been carriers but had completed these tests. The results were so marked in some cases that the whole experiment was continued on throughout the year.

It was soon obvious that there is no definite reaction to high-titre serum, nor is there any theoretical reason for a true anaphylactic reaction and the serum application was abandoned. The experiment was therefore continued only as to the local reactions to toxins of dead cultures of a mixture of enteric group organisms and a control. As the year passed these cases became really interesting in that they appeared to be giving definite results. The detailed table is given below.

The actual method is carried out as follows: Three platinum loopfuls of the following substances are placed on the forearm, the skin of which has been thoroughly cleansed with spirit; and an area of about one inch in diameter is scarified through the liquid, which is then allowed to dry, and no dressing applied.

The three substances used at first were: (*a*) Sterilized normal saline as a control; (*b*) a mixed high-titre serum of *Bacillus typhosus*, para A and para B; (*c*) a mixed killed culture of *B. typhosus*, para A and para B of about 200 millions per cubic centimetre. After a short time the ordinary T.A.B. vaccine was substituted for this.

Where only two lines of results are given in column 5, these are always the results of (*a*) the control, and (*c*) the T.A.B. vaccine. Throughout the series (*a*) and (*c*) should be taken as giving the actual result. The reaction is noted every twenty-four hours until it finally disappears and is read as follows:—

‡ Full red reaction with raised surface and œdema of area round.
\+ Full red reaction extending beyond the area of scarification.
± Red area of scarification.
— No reaction beyond scratch marks.

The times given in column 3 are for convalescents from the date of normal temperature; and for carriers from the date of finding.

Comments on Cases.

The cases can be divided into the following groups:—

(*a*) Cases 1 to 6 are those which actually were detected or relapsed during or after testing. It will be noted that in every case a full reaction for at least four days was given at the first time of experiment, and that every case, except No. 1, gave a reaction only the first day for the second experiment which was made after all tests had been completed and the case declared free from infection in accordance with the ordinary rules.

Case 1 is of great interest in that it was one of the first cases tested and looked like an obvious positive, but was never detected and was declared free. This looked decidedly disappointing in that one of the first cases which was probably positive from this cutaneous reaction should prove negative from the routine tests as carried out for all convalescents and made it appear that this test would not prove reliable. However nine months later he relapsed with a severe attack of para A fever; the bacillus being isolated from the stools on several occasions. A second experiment was made after the last finding and as shown again appears to be positive. Thus it appears that he was during this whole period really a carrier of para A, although not detected on urinary and fæcal tests. It appears to be unlikely that the second attack was a fresh infection.

Case 6 is also interesting in that he gave a very marked reaction (it should be noted that he has a very fair skin) and yet proved negative on ordinary tests; but owing to the reaction being so strong, tests were continued on beyond the official twenty and colonies put up from the twenty-second test proved positive for para A. As will be seen, the second experiment made on this case about four months later is an obvious negative.

(*b*) Cases 7 to 9. Three officers who had all been carriers but who had completed their tests some time (two to three months) previously and had been declared free. It will be seen that they all proved to be negatives.

Case 9 is interesting in that he had never knowingly had any kind of enteric group disease and had originally been detected as a carrier when his urine was being tested for organisms after smallpox.

(*c*) Cases 10 to 40 are all cases of ordinary enteric convalescents who were being tested in the ordinary routine course, and who were never detected as carriers. The experiment in nearly every case was done within the first week after arrival at the depot, i.e., when full convalescence had set in and the patient was on ordinary diet. It will be noticed that they are all obvious negatives with the following exceptions:—

Case 13 always looked ill and the experiment was made in very early convalescence.

Case 15 gave a long lasting but slight reaction, and was never detected.

Case	Type			Time since—		Inoculated	Readings	Results	Remarks
	Carrier	Convalescent	Control	Illness	Convalescence				
(1) F.I. Offi. K.	..	Para A ..	..	5 months..	..	Yes, 3 years	− − − − − − (a) − − − − − − (b) − + ‡ + − − (c)	Five tests negative	Always suspected of being a carrier, but tests negative. Relapsed 8 months. Para A
	Para A ..	..	..	1 month ..	..	Yes, 4 years	− − − − − ‡ ‡ ‡ + −	Ten tests negative to date	Para A in stools three times since lysis. Experiment 7 days later
(2) Cpl. M...	*B. typhosus*	..	..	6 months..	2 months..	Yes, 8 months	− − − − − − − − − + − − − − − − + ‡ ‡ + ± ± − −	Forty tests negative	Detected carrier three times and on tests of long period. Experiment repeated after completion of tests with negative results as shown. Passed free
	..	..	..	..	..	..	− − − − ± − − − + ± − −	..	
(3) Seaman J.	..	Para A ..	..	1 month ..	..	Yes, 8 months	− − − − − − − + + ± − − − − ‡ ‡ + ± ± − −	Positive 20 days later	Always looked ill and a detected carrier. Tests recommenced a month later and experiment repeated during these. Passed free
	Para A ..	..	..	3 months..	1 month ..	Yes, 1 month	− − − − − − − −	Twenty-five tests completed negative	

(4) Pte. B. ..	..	Para A ..	..	2 months..	..	No	– – – – – – – –	(a)	Detected carrier twice	Detected a Para A carrier. Tests recommenced and again detected; but this time *B. typhosus* tests recommenced a month later and experiment repeated after completion. Passed free
							+ ‡ ‡ + ± ± – –	(b)		
								(c)		
	Para A *B. typhosus*	..	..	6 months..	1 month ..	..	± – – –		Thirty tests negative	
							‡ – – –			
(5) Pte. S. ..	..	Para A ..	..	2 months..	..	Yes, 4 months	– – – – – –		Detected carrier	Detected after ten tests. Started tests again 3 weeks later and experiment repeated with the results shown. Passed free
							+ ‡ + ± – –			
	Para A ..	..	..	..	..	..	– – – –		..	
							± – – –			
(6) Lieut. B.	..	Para A ..	..	3 months..	..	Yes, 1 year	± – – – – – –		Detected after twenty-two tests	Never looked ill or like a carrier. Tests restarted and experiment repeated 4 months later with obviously negative results
							‡ ‡ + ‡ ± – –			
	Para A. ..	..	..	..	1 month ..	..	– – – –		Tests all negative	
							± – – –			
(7) Lieut. B.	Para A ..	..	..	8 months..	4 months..	Yes, 1½ years	– – – –		Tests negative	Had been carrier 4 and 5 months previously
							+ ± – –			
							± – – –			
(8) Capt. M.	Para A ..	..	..	8 months..	6 months..	Yes, 4 years	– – – –		Tests negative	Had been carrier 4 and 5 months previously
							– – – –			
							– – – –			

Case	Type			Time since		Inoculated	Readings	Results	Remarks
	Carrier	Convalescent	Control	Illness	Convalescence				
(9) Lieut. S.	*B. typhosus*	..	..	..	5 months..	Yes, 1 year	− − − − (*a*) ± − − − (*b*) + ± − − (*c*)	All tests negative	Had never had enteric to his knowledge. Detected *B. typhosus* carrier. Passed free
(10) Lieut. R.	..	Enteric group	..	3 months..	..	Yes, 1½ years	− − − − + − − − − ± − −	Tests negative	..
(11) Lieut. A.	..	Enteric group	..	3 months..	..	No	− − − + − − + − −	Tests negative	..
(12) Lieut. S.	..	Typhosus	..	5 months..	..	Yes, 3 years	− − − − − − − − − ± − −	Tests negative	Had completed tests 1 month previously and passed free
(13) Serjt. P.	..	Enteric group	..	1 month ..	..	Yes, 6 months	− − − − − − + ± − − − − + + ± ± − −	Tests negative	This case looked ill and experiment was done in very early convalescence
(14) Bdr. R...	..	Para A	..	3 months..	..	Yes, 8 months	− − − − − + − − − − ‡ + ± − −	Tests negative	..
(15) Pte. F...	..	Para A	..	4 months..	..	Yes, 5 months	− − − − − − − − − − − − − − − − − ± ± ± ± − −	Tests negative	Case viewed with suspicion, but never detected. Passed free
(16) Pte. C. ..	..	P.U.O.	..	3 weeks ..	..	Yes, 3 months	− − − − − − − − − + + ± − − − − − + + + ‡ ± + − −	Tests negative	A suggestive case, but never detected; looked ill and had only been 3 weeks in bed, and then sent straight to depot. Second experiment 8 months later negative
	..	..	..	9 months..	..	11 months	− − − − + − − −	..	

(17) Lieut. H.	..	Enteric group	..	2 months..	..	Yes, 1 year	– – – – + – – –	Tests negative	..
(18) Boy L. ..	..	Para A	..	2 months..	..	Yes, 7 months	– – – – + ± – –	Tests negative	..
(19) Lieut. E.	..	Enteric group	..	3 months..	..	Yes, 4 years	± – – – – + + ± – –	Tests negative	Always looked ill; never detected. Severe appendicitis 8 months later
(20) Lieut. —.	..	Para A	..	3 months..	..	Yes, 11 months	– – – – ± – – ± – –	Tests negative	..
(21) Pte. E...	..	Para A	..	3 months..	..	Yes, 3 months	– – – – – –	Tests negative	..
(22) Pte. S. ..	..	*B. typhosus*	..	1 month ..	..	Yes, 5 months	± – – – ‡ – – –	Tests negative	..
(23) Pte. B. ..	..	*B. typhosus*	..	2 months..	..	Yes, 5 months	– – – ± – –	Tests negative	..
(24) Drvr. F.	..	Para A	..	2 months..	..	Yes, 9 months	± – – – + + – –	Tests negative	..
(25) Pte. W...	..	Para A	..	1 month ..	..	Yes, 1 year	± – – – – ± – –	Tests negative	..
(26) Lieut. M.	..	Para A	..	2 months..	..	Yes, 1¾ years	– – – + – – –	Tests negative	..
(27) Cpl. Y...	..	Para A	..	1 month ..	..	Yes, 10 months	– – – – ‡ + – –	Tests negative	..

Case	Type			Time—since		Inoculated	Readings	Results	Remarks
	Carrier	Convalescent	Control	Illness	Convalescence				
(28) Bdr. C...	..	*B. typhosus*	..	2 months ..	..	Yes, 3 years	– – – – – (a) (b) + ± – – (c)	Tests negative	..
(29) Gnr. M...	..	Enteric group	..	2 months ..	..	Yes, 6 months	± – – – ‡ ± – –	Tests negative	..
(30) Pte. S. ..	..	Para A ..	..	1 month ..	..	Yes, 2 years	– – – – ‡ – –	Tests negative	..
(31) Clerk H.	..	Enteric ..	..	1 month ..	..	Yes, 9 months	– – – – – – + + ± – –	Tests negative	A Eurasian clerk who always looked ill and had had an enteric leg; never detected
(32) Pte. M...	..	Para A ..	..	1 month ..	..	Yes, 1 year	± – – – ‡ + – –	Tests negative	..
(33) Clerk F.	..	*B. typhosus*	..	4 months ..	..	Yes, 2 years	– – – – – – ± + + – –	Tests negative	An Active Service case with some œdema of leg; never detected
(34) F. I. Offi. D.	..	Para A ..	..	2 months ..	..	Yes, 2 years	– – – – – –	Negative	..
(35) Gnr. H.	..	Para A ..	..	3 months..	..	No	– – – – ‡ ± – –	Tests negative to date	..
(36) Pte. S. ..	..	Para A ..	..	2 months ..	..	Yes, 2 years	– – – – ± + – –	Tests negative	

(37) Gnr. C.	..	Para A ..	..	2 months..	..	Yes, 1 year	−−−− +±−−	Tests negative	..
(38) Pte. R...	..	Enteric group	..	3 months..	..	Yes, 5 months	−−− +−−	Tests negative	..
(39) Pte. M...	..	Enteric group	..	1 month ..	..	Yes, 1 year	−−− ±−−	Tests negative	..
(40) Major D.	..	Para A ..	..	1 month ..	..	Yes, 2 years	−−− ±−−	Tests negative	..
(41) Capt. T.	..	Enteric ..	Control	22 years ..	..	Yes, 6 months	−−−− +±−−	..	Thought to have been a carrier for ten years
(42) Asst. Surg. N.	..	..	Control	..	..	Yes, 7 years	−−−−− ‡ ++−−	..	Has been working in an enteric laboratory three years
(43) Cpl. C. ..	..	..	Control	..	..	Yes, 6 months	−−− +−−	..	A dysentery case
(44) Cpl. G. ..	..	..	Control	..	..	Yes, 3 years	−−− ±−−	..	A dysentery case
(45) Sapper R.	..	..	Control	..	..	Yes, 1 year	−−− +−−	..	A dysentery carrier
(46) Sapper N.	..	..	Control	..	..	Yes, 4 months	−−− ±−−	..	A dysentery case
(47) Cpl. E. ..	..	..	Control	..	..	Yes, 5 months	−−−− ±±−−	..	A dysentery case
(48) Pte. R. ..	..	..	Control	..	..	Yes, 2 months	−−−−− +±±−−	..	A dysentery case

In addition Cases 49 to 60 are also controls, chiefly from dysentery cases, giving approximately the same reactions, namely those inoculated within six months, give a reaction into the second day, while those who have not been examined for 9 to 10 months give practically no reaction beyond the first day.

Case 16 in my opinion was an obvious carrier. He looked ill and had only been kept in hospital about three weeks or so, and the experiment was made in a very early stage of convalescence; but twenty-five tests proved negative. A second experiment was made about nine months later and the result is shown; from this result he would appear to be now a negative case, and some change has evidently taken place in his response to enteric group toxins. This appears to be highly suggestive that at the time of the first experiment he was really a group carrier although never detected, and that at the second time he had completely recovered from his infection.

Case 19 gave a possibly positive reaction. He always looked ill and after returning to his unit, he had a severe attack of appendicitis and this may have accounted for his enhanced reaction to the para A toxin. The cause of the appendicitis was not ascertained.

Cases 31 and 33 were European clerks: both looked ill the whole time during which tests were being made and the second had had a "typhoid leg." There was no opportunity of repeating the experiment after the completion of tests.

Cases 41 to 60. These were control cases taken chiefly from dysentery convalescents and had never had any enteric symptom. Nearly half of these had been inoculated within six months previously, the remainder had been inoculated more than six months before. The different results of these cases are typified by cases 43 to 45 and cases 46 to 48. The latter, i.e., recently inoculated, gave a definitely stronger reaction which lasted into the second and in one even into the third day.

Case 41 had possibly been a carrier for about ten years—during boyhood, as a positive Widal reaction was given ten years after an attack of enteric group illness without any intermediate T.A.B. inoculations and with periodic attacks of migraine during the ten years. A T.A.B. inoculation had been given about six months previously.

Case 42 is interesting in that he had never had enteric group infection of any kind and had not been inoculated for seven years. He had however been working in an enteric laboratory during the previous three years, and it is possible that this might have some effect from unconscious inoculation.

From a review of the above cases it would appear that there are distinct possibilities in this cutaneous method of detection of enteric group carriers and that it may even prove more reliable than urinary and fæcal testing, for example as in the case of No. 1, No. 15 and No. 16, all of which were negative on ordinary testings but appears positive by this cutaneous test. In the case of No. 1 this cutaneous test appeared to be proved correct after all.

A positive result appears to be the continuance of the "red reaction" into the fourth day or more, while any case definitely continuing into the third day should be regarded with suspicion; but in the latter case, recent inoculation with T.A.B., say within five to six months, or very early convalescence, should be taken into consideration. A further series is being started to test these factors. In the series of cases present, the majority of experiments were made soon after arrival of the patient in the depot and the commencement of tests, i.e., on an average eight weeks after the fall of temperature.

The history of other diseases such as malaria, dysentery, etc., does not appear

to have any influence on the results, as a careful comparison of cases with different past illnesses shows no apparent differences in the reactions.

One point which apparently influences the reaction is the type of skin and complexion; a fair-skinned pink individual giving a $\pm$ reaction even with the saline control and consequently a more showy reaction with the inoculating media. A case in point was Case 6, but Case 16 which appeared to be positive from the reactions given and which was never found actually to be a carrier was not of this type. Particular note of this factor will be made in the new series of experiments about to be commenced.

The only reference to a similar type of experiment or reaction to be found in the literature available is that given above. In this an effort was made to obtain a reaction which would determine the duration of the immunity effects of T.A.B. inoculations for purposes of reinoculation. From a consideration of these cases, and of the fact that a rising Widal is only obtained in the later stages of enteric diseases, it would appear probable that a skin reaction will only be obtained in the late stages of the disease or in early convalescence, or in a persistence of the invading organism, as in the case of a carrier.

On the whole, the results are most interesting, and appear to be a fairly reliable guide. The number of cases is small for anything like a definite conclusion, but no more cases were available during the year. Whether a positive reaction is accompanied by a continued high Widal is unfortunately not ascertained; and whether any further differentiation could be made with separate cultures of *B. typhosus*, para A and para B, was unfortunately not tried. If this prove to be so, then there should be further possibilities of differential diagnosis of P.U.O. and enteric group diseases. This might be especially useful in cases admitted too late for positive blood culture and when no definite diagnosis had been reached in obscure fever cases.

Conclusions.

The reactions and findings of this experiment, taken as a whole, are apparently reliable and satisfactory, but the numbers available are very small, and it would be interesting to see the results of a much larger number for corroboration.

The experiment appears to offer an easy and quick method for the detection of enteric group carriers, and possibly of diagnosis in late stages of the disease, by by the use of separate cultures of *B. typhosus*, para A and para B: this point, however, requires further testing on a number of cases. The test should prove valuable, when thoroughly corroborated, in finding the carrier source of sporadic outbreaks of enteric group fevers in any community, either military or civil, since all possible human sources can readily be tested and suspicious cases ear-marked for further investigation. It should simplify the examination of the kitchen staff of any establishment either prior to engagement or during routine examinations. It should also be a considerable help in enteric and other convalescent depots for the rapid detection of enteric group carriers and the early return of non-carriers to their own units, especially during the urgent demands of active service.

DEATH FROM SPONTANEOUS HÆMORRHAGES.

By Captain F. W. HARLOW.
Royal Army Medical Corps.

Among the many interesting pathological conditions of the blood, that of idiopathic spontaneous hæmorrhage has always attracted considerable attention. This may be attributed chiefly to its dramatic and alarming symptoms, to the difficulty of early diagnosis, and also to the somewhat limited amount of exact knowledge of the pathological changes underlying the disease of which it is a symptom.

A case illustrating very clearly the difficulties of diagnosis when the other symptoms are atypical, came under my care recently. I regret that circumstances, the nature of which I am not at liberty to define, prevented me from making a complete pathological investigation. This inability to investigate is particularly disappointing because it is unlikely that similar cases will be encountered frequently. However, the clinical and such pathological features as could definitely be established are set down for consideration.

J. S., male, unmarried; aged 26; had for some three months been living a healthy open air life and was in normal health; previous to this he had gone through a period of fairly considerable mental and physical strain.

On April 20, 1921, he consulted a doctor, complaining of indisposition, and discomfort in the epigastrium after meals. His temperature and pulse-rate were normal, and, as he had a hearty appetite, his complaints were put down to over-indulgence in this respect combined with lack of exercise. He was treated accordingly.

He came under my care five days later; his temperature had then risen for the first time, being 99·2° F.; his pulse-rate was seventy-two per minute and he had all the signs and symptoms of acute gastritis. He was confined to bed and treated in the routine way.

On April 28, 1921, his condition had improved somewhat, there being only slight tenderness in his epigastrium on palpation, and he stated that his nausea had practically disappeared.

During the first three days that he was under my care it was observed that he was displaying more anxiety and depression than his disease would warrant. Subsequent investigation proved that there was adequate reason for this mental condition and that it was not caused only by his illness or fears of possible disease.

As his bowels were confined he was given a soap and water enema. The result was quite normal, with no evidence of melæna. He vomited once or twice during the evening, but there was no hæmatemesis.

The next day he complained of severe malaise although his temperature and pulse rate were only slightly above normal; apart from this the symptoms remained unchanged. That evening he vomited mucus several times and it was noticed that the vomit was streaked with fresh blood. The source of this was discovered in small points of hæmorrhage on his gums and palate.

The vomiting abated somewhat and the bleeding from his gums ceased during the morning of May 1, 1921. Later in the day he had a typical coffee-ground

hæmatemesis and became unable to retain anything but soda-water in his stomach. The hæmorrhages from his gums and palate then re-commenced. His abdomen was quite flaccid and his epigastrium still not markedly tender on palpation. His liver and spleen were normal in size and position, and there was neither ascites nor œdema of face or legs.

After this hæmatemesis the patient felt easier and his general condition and flaccid abdomen did not suggest any cause for anxiety. In a short time he went to sleep and slept until the following morning.

On May 2, 1921, he vomited once during the morning, bringing up only thin watery mucus, but the points of hæmorrhage on his gums were still evident. The patient was interrogated as to any previous or family history suggestive of hæmophilia, but his answers and such subsequent investigations as I was able to make revealed nothing of this nature.

It should be pointed out that his answers were not very satisfactory, nor were adequate investigations as to his family history possible, owing to the fact that only distant relatives could be approached, his home being in another part of the country. Such investigations however proved negative.

Early in the evening the patient had a profuse hæmaturia on micturition. The urine on examination contained hæmoglobin and albumin in addition to blood. This condition was treated with local adrenalin (one drachm) passed into the bladder by a soft catheter and washed out fifteen minutes later with boric lotion. A hypodermic of morphia and adrenalin was then given.

Later a severe rectal hæmorrhage occurred on evacuation of his bowels. This was followed by another hæmatemesis of fresh blood. Twenty cubic centimetres of anti-tetanic serum as a substitute for horse serum, which was not available, were injected subcutaneously. A rectal saline injection was then commenced and two pints were absorbed before death occurred.

His condition then improved slightly for a few hours, but at 3 a.m. the next day another severe hæmatemesis occurred, followed by signs of severe shock. It therefore appeared probable that an internal hæmorrhage was occurring.

Air-hunger then became very evident. Despite morphia and adrenalin hypodermically and oxygen inhalations he died at 5.30 a.m.

There are many diseases during the course of which death from spontaneous hæmorrhage may occur.

It is not however proposed to consider all of these, but only such as would provide a satisfactory diagnosis for the case just recorded.

A malarious or other parasitic origin was improbable owing to the fact that the patient had never been out of the United Kingdom in his life; nor were there any signs of syphilis.

The diseases mentioned in the following paragraphs are arranged in order, commencing with those considered to be least probable.

Before discussing any of these it should be borne in mind that no cutaneous changes in the nature of rash or abnormal coloration were evident, until the pallor of shock made its appearance. This fact alone added greatly to the difficulty of diagnosis.

Cases of death from hæmorrhage have been recorded in two diseases, namely, portal cirrhosis and Hanot's disease or biliary cirrhosis, neither of which are primary blood diseases.

In the atrophic type of portal cirrhosis described by Laennec, the liver is not enlarged. The absence of the following symptoms: enlargement of the spleen, distended epigastric or mammary veins, ascites and an alcoholic history at once disposes of this diagnosis.

Some of the prominent features of Hanot's disease are enlargement of both liver and spleen, jaundice, and the presence of bile in the urine; but none of these features were present in the case just described.

Osler records three cases of splenic anæmia or Banti's disease where the hæmorrhages were so severe as to be the actual cause of death. One of the cardinal signs of this disease is enlargement of the spleen and sometimes at a later stage the liver. This as a diagnosis is therefore inadmissable.

Acute leukæmia or leucocythæmia of both the lymphatic and splenic varieties occasionally runs a fairly rapid course.

Two cases are described by Osler in which severe hæmatemesis was not only the first symptom but also the cause of death.

In the lymphatic form especially, the enlargement of the spleen is sometimes quite slight. In very rare cases of this type enlargement of the lymphatic glands is scarcely evident.

Hæmaturia and hæmoglobinuria are on the other hand rare accompaniments of this disease. Anæmia is not invariable and the most common form of hæmorrhage is epistaxis.

Important signs of the lymphatic type are angina, involving frequently both the tonsils and pharynx, and cutaneous hæmorrhage. Acute cases of the splenic variety usually last three months, although the onset may be so insidious as to prevent the patient coming under observation until quite late in the course of the disease, but at this stage some enlargement of the spleen would be expected. It would not therefore appear likely that this was the disease from which the patient was suffering.

Death without remissions often occurs with considerable rapidity in cases of pernicious anæmia of the aplastic form. One case is recorded where death took place ten days after the first onset of symptoms. Abnormal coloration of the skin (lemon, icteroid, brownish or even pallor) is an important sign of this condition. In the case in point, pallor was not present to any marked extent until the onset of shock a few hours before death.

Anæmia, palpitation, breathlessness and muscular weakness, which also characterize this disease, were not at first evident in the case under discussion.

Appearing later they were explicable on the grounds of a secondary anæmia from hæmorrhage.

Cases of scurvy, occurring among prisoners even when the diet was amply sufficient, are described by Osler. Mental strain, depression and nostalgia are causal factors but they are usually combined with poor living conditions, overcrowding and fatigue. In this case the anti-scorbutic part of the diet consisted of fresh meat three pounds, fresh vegetables seven pounds, and fresh fish three-quarter pound per week; and overcrowding and fatigue were certainly not factors in the ætiology. This disease has now become very rare in the United Kingdom. The rapid transition from slight indisposition to severe illness and death without previous wasting, petechial ecchymoses, swelling of the tongue, hæmorrhage under the buccal mucous membrane or œdema of the ankles should be enough

to differentiate the case from this condition. In severe cases death from hæmorrhage does occur. Hæmaturia is however a very uncommon symptom. In cases of such severity some signs of scurvy, sclerosis or hæmorrhagic periosteal nodes might reasonably be anticipated. This diagnosis is in consequence untenable.

This case must therefore be classed as one of the hæmorrhagic diathesis. According to W. Koch all cases from the mildest purpura simplex to the most severe purpura hæmorrhagica and hæmophilia are included under this heading.

Purpura fulminans is most commonly a disease of childhood and is characterized chiefly by cutaneous hæmorrhages. Death may and frequently does occur before any severe spontaneous hæmorrhage has taken place.

The case appears to correspond most nearly to purpura hæmorrhagica or hæmophilia. In the latter condition "family history is in very rare cases lacking" (Osler).

Bullock and Fildes state that: "The cardinal symptoms of hæmophilia are three in number . . . . an inherited tendency in males to bleed. No solitary hæmorrhage, however inexplicable, should in our opinion be regarded as hæmophilia; it is necessary to show that the individual has been repeatedly attacked if not from birth from infancy."

The absence of a history of previous hæmorrhage is therefore the only obstacle to hæmophilia as a diagnosis.

"Patients are often sensitive and reticent regarding the fact of their tendency to bleed; and it may only be elicited after close investigation" (Miles and Thomson). When the patient was last interrogated he was well aware of the gravity of his condition, and in all probability ignorant of the fact that reticence might hinder his adequate treatment and possible recovery.

The majority of hæmorrhages in this disease are traceable to trauma, but spontaneous bleeding may occur.

To base a diagnosis on the possibility of the patient's reticience alone is extremely unsatisfactory.

Non-infective purpura hæmorrhagica is the only remaining disease in this group to which the case reported bears any resemblance. This usually attacks young subjects and is therefore considered by some to be an acute form of hæmophilia.

A severe purpuric eruption is stated always to precede the bleeding from mucous or any other surfaces. This rash is not a transient one. The fact that two other members of the profession in addition to myself were for the last three days constantly on the look out for any such cutaneous signs quite negatives the possibility that they were overlooked.

This condition must then have been one of purpura hæmorrhagica unaccompanied by any cutaneous extravasation. It was considered worth recording the facts of this case, in which such a rapid fatal issue occurred with no signs of the characteristic rash, as no mention was found of any similar case in the literature dealing with diseases of this nature.

Current Literature.

The Assessment of Physical Fitness. By George Dreyer, C.B.E., M.A., M.D., in collaboration with George Fulford Hanson. London (Cassell) 1920. Pp. 115.—The publication of the Ministry of Service's Report on the physical examination of men of military age (Cmd. 504 of 1920), naturally attracted much attention to the subject of the physical fitness of the nation and the effects thereon of environmental conditions and habits of life. The obvious and cogent criticism to which that Report was open was that it dealt with a population which had already experienced a severe physical weeding-out so that the results were not of general application. Captain Agnew's Report on the health and physical condition of male munition workers (published on p. 87 *et seq.* of Cd. 8,511 of 1917), the Interim Report of the Health of Munition Workers Committee was not so exceptional from that point of view, but dealt with a comparatively small sample of males and recorded few exact physical measurements. Obviously what is needed is a test easy of application and therefore fit to be applied to large samples of the whole population.

In 1844 (*Journal of Statistical Society of London*, vol. vii, p. 193), the late Dr. John Hutchinson proposed inter alia, to assess physical capacity by means of the spirometer, and published many observations upon healthy and diseased persons. Professor Dreyer (whose book is appropriately dedicated to the memory of Hutchinson) has taken up the problem and now published a collection of tables to be used for the following purposes:—

(1) The determination of what are the normal proportions between the weight, the trunk-length and the circumference of the chest.

(2) To gain evidence as to underfeeding or malnutrition during different stages of adolescent or adult life, as well as in various classes and occupations.

(3) For the study of the different aspects of physical fitness as measured by vital capacity, in its relation to weight, trunk-length and chest circumference.

(4) The application of these various measurements to patients with organic diseases, e.g., pulmonary tuberculosis, as well as persons with functional disorders. The method of procedure is the following. The sitting height is determined (trunk length) and the circumference of the normally breathing not expanded chest. The weight is then compared with that deduced by Professor Dreyer's formulæ from the other two measurements.

We next have tables of vital capacities for each of the three classes (A) representing perfect, (B) medium, and (C) poor physical fitness. An example will show how Professor Dreyer proceeds.

A certain male, aged 25, had the following measurements. Weight 59·0 kilos. Length of trunk 88·1 centimetres. Circumference of chest 85·0 centimetres. Vital capacity 4,200 cubic centimetres. Calculated by tables from length of trunk, the weight should be 61·65 kilos. Calculated from circumference of chest it should be 62·45 kilos. The average of these, 62 kilos, is taken as the proper weight for the subject, so he is 7 multiplied by 100 and divided by 62, that is 11·29 per cent overweight. The vital capacity calculated from weight will obviously be too large on account of the person being above weight. The vital capacity from length of trunk for Class A would be 4,073 cubic centimetres, from circumference of chest 4,109 cubic centimetres. The average is 4,091, and the observed vital capacity is 2·66 per cent greater than this. We have quoted one of Professor Dreyer's examples in full. The book being a practical manual does not enter into a justification of the precise formulæ chosen, nor explain why, for instance, the assessment of vital capacity should not be based independently upon the weight as well as upon the other dimensions. But the author has expounded

his theories elsewhere and, from the introduction to the present volume, it is evident that his standards have been tested upon large samples of healthy persons. Consequently the method deserves full trial and its simplicity will permit of such a trial being carried out. It is to be hoped that data will be assembled representative of the average conditions in different localities and industries; the importance of the work from the standpoint of preventive medicine is very great.

The Treatment of Hepatic Abscess with Emetine. By Vincent S. Hodson, M.B., M.V.O., Director, Khartoum and Omdurman Civil Hospitals, from the *Journal of Tropical Medicine and Hygiene.*—The author describes the treatment of one case with emetine in which an abscess had ruptured through the lung, and three cases in which there were all the symptoms and signs of hepatic abscess and in which the exhibition of emetine reduced the swelling, relieved the tenderness, and the patients put on weight. The author in continuation says that although this treatment is not in accordance with that laid down in the latest edition of Manson's "Tropical Medicine," yet he considers that the results obtained in his cases justify its repetition even though the amœbic hepatitis has proceeded to abscess formation. As is well known the pus obtained from an amœbic abscess does not usually contain amœbæ for the first day or two because they are in among the cells which have not yet shredded off into the abscess cavity. Although there is no definite abscess wall, there is undoubtedly an attempt made to localize the abscess and to prevent absorption of its contents. Liver abscess cases are often afebrile for varying periods, and the author finds that the giving of emetine produces a reaction which is of use in confirming the diagnosis. If the amœbæ are killed, as seems to be the case when emetine is given, he sees no reason why the contents of this peculiar type of abscess should not be absorbed. The reaction which commonly takes place when emetine is given, he regards as marking the commencement of the absorption of these contents.

The author finally states that his intention for the future is not to operate in cases of amœbic abscess until he has given emetine a really good trial.

Experimental Studies of the Nasopharyngeal Secretions from Influenza Patients. (I.) Transmission Experiments with Nasopharyngeal Washings. By Peter K. Olitsky, M.D., and F. L. Gates, M.D., *Journal of Experimental Medicine,* February 1, 1921, vol. xxxiii, No. 2, Pp. 125-145.—This study was made during the course of over one and a half years, in three successive periods. The first period coincided with the epidemic wave of 1918-19. During this period cases of acute uncomplicated influenza were studied.

The second period embraced the late autumn of 1919, when influenza did not prevail in New York in epidemic form. During this period normal individuals were studied as controls.

The third period—the winter of 1920—saw a return of the epidemic. Additional cases were then studied.

Materials.—Acute cases were selected during the early part of the epidemic period. Considerable stress, as a diagnostic point, was laid upon the presence of a leucopenia especially affecting the mononuclear cells—this in conjunction with the usual clinical symptoms of epidemic influenza.

Saline washings from the nose and throat were employed. Material was obtained from eight cases of influenza within the first thirty-six hours of the disease, and from twelve cases at later stages, including convalescence or the period of post-influenzal pneumonia. In addition, fourteen individuals who had not been affected were tested during the epidemic or interepidemic period.

Rabbits were chosen as experimental animals. All animals were subjected to preliminary blood counting, weighing and temperature taking, and any showing variations beyond the average were rejected.

Inoculations were made directly into the lungs by means of an intratracheal catheter, or by tracheotomy.

Materials Inoculated.—(*a*) Unfiltered nasopharyngeal washings; (*b*) filtered washings; (*c*) lung-tissue suspensions, filtered and unfiltered, from previously inoculated rabbits; (*d*) similar lung tissue preserved in sterile fifty per cent glycerol; (*e*) bacteria and culture materials; (*f*) control material.

Usual dose for 2·5 to 3 kilogram rabbit was three cubic centimetres of these materials.

Inoculation of Unfiltered Washings.—Material from eight cases of uncomplicated influenza taken during the first thirty-six hours of the disease, and from twelve cases in the later stages of the disease, was employed.

Seven of the eight early cases gave rise to the following train of symptoms in the rabbits; none of the material from the twelve late cases had any such effect. After an incubation period of twenty-four to forty-eight hours, fever and general malaise developed, and the striking feature was a definite and often marked leucopenia resulting from depression of the mononuclear cells. This disappeared in the course of two or three days, and all the animals recovered except one or two which succumbed to bacterial infection. Some of the animals were killed on the second day of the disease, and showed œdema and emphysema of the lungs with a mottled hæmorrhagic appearance. The hæmorrhages on the surface beneath the pleura were diffuse or discrete, occupying areas a few millimetres in extent, or covering a large part of a lobe. In addition, minute petechiæ were scattered over the entire surface. On section of the lungs the cut surface revealed a hæmorrhagic œdema; it dripped a blood-stained frothy fluid.

On microscopic section the lesions were found to consist of (*a*) hæmorrhagic foci, and (*b*) œdema and emphysema. The alveoli contained coagulated serum or red corpuscles, mononuclear cells, and also, at times, polymorphonuclear cells of eosinophile type.

No ordinary bacteria were seen in impression films of the lung tissue or in aerobic or anaerobic cultures of the tissue.

The animals were killed by a sharp blow and not by ether or chloroform.

Successive inoculations of bacteria-free emulsified lung tissue of rabbits produced similar appearances in the lungs of inoculated animals up to the eleventh passage; when the series was discontinued. Passages first, fourth and eighth showed bacterial infection also, due to pneumococcus, Type IV, and *Micrococcus catarrhalis;* but the pneumonic areas were limited to small areas of one lung.

Experiments carried out with material obtained from cases of influenza of the 1920 epidemic gave similar results to the above when inoculated into rabbits, both with filtered and unfiltered washings.

Several cases later in the disease or in convalescence were also investigated on similar lines, but none of the animals used showed the condition above described.

Controls were also carried out by the injection into the lungs of rabbits of saline solution, suspension of normal rabbit lungs, normal rabbit serum, foreign protein such as human ascitic fluid, bacteria of the ordinary species, including Pfeiffer's bacillus and its poison, and finally the nasopharyngeal secretions from fourteen persons free from influenza and tested in the epidemic and interepidemic periods—seven of the latter were suffering from coryza. None of the fifty-five animals employed showed the changes described as occurring in the animals inoculated with material obtained from influenza cases in the early stages.

The conclusions arrived at were that patients with epidemic influenza—at least in the early stages of the disease—carry in their nasopharyngeal secretions a substance which is not ordinary bacteria or their metabolic products.

This substance when inoculated intratracheally into rabbits readily causes fever, leucocytic, and particularly mononuclear cell depression, lung hæmorrhages, œdema and emphysema,

This substance apparently disappears from the discharges of such patients about thirty-six hours after the symptoms of the disease have appeared, and is not found in healthy people or in other pathological conditions. It is filterable.

D. H.

Reviews.

AUTOEROTIC PHENOMENA IN ADOLESCENCE. AN ANALYTICAL STUDY OF THE PSYCHOLOGY AND PSYCHOPATHOLOGY OF ONANISM. By K. Menzies. London: H. K. Lewis and Co., Ltd., 1921. Pp. viii + 100. Price 5s. net.

This essay deals with a practice which, though undoubtedly common, is rarely discussed in medical literature. On the contrary, it has unfortunately largely become the province of the quack or of the unwittingly ignorant.

The general thesis is well presented, and the rational view as to the absence of any basic hurt sustained. As the author holds that the principal evil results of excess are on the psychic side, it is but natural that he turns to psycho-analytical principles for treatment.

The book is one that would prove useful to anyone having to do with the training of youth.

THE CLINICAL EXAMINATION OF THE NERVOUS SYSTEM. By G. H. Monrad-Krohn, M.D.Christiania, M.R.C.P.Lond., M.R.C.S.Eng. London: H. K. Lewis and Co., Ltd., 1921. Pp. xv + 135. Price 6s. net.

In this little book the scheme outlined follows in the main the routine adopted in British neurological clinics, and as such affords a handy reminder to those not regularly engaged in such examinations.

The relation of psychiatry to neurology is indicated, the author taking the generally accepted line that though an investigation of the mental condition of the patient is an essential part of the routine examination, psycho-analysis should remain the province of a few.

There are some omissions, apparently intentional as being of less practical importance, and the book would gain by the addition of an illustrated chapter on the anatomy of the nervous system.

It is on the whole, however, a simple and clear exposition of the essential tests that should be applied in all nerve cases, the diagnosis of which, perhaps more than any other variety, is dependent on accurate anatomical knowledge and routine examination.

DISEASES OF THE NERVOUS SYSTEM. By H. Campbell Thompson, M.D.Lond., F.R.C.P. London: Cassell and Co., 1921. Pp. xvii + 566. 3rd Edition. Price 15s. net.

Filling the gap between the necessarily limited description of nerve diseases found in the ordinary student's manual and the more elaborate textbooks on neurology, this work serves a useful purpose. The accounts of the various diseases are clear, and not overburdened with detail. An excellent feature is the introduction at the beginning of each study of an anatomical description of the part of the nervous system involved. Were this amplified by the introduction of more diagrams and schematic representations, the book would materially gain in value.

Notices.

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Original Communications.

AN INTRODUCTION TO THE STUDY OF HÆMATOPHAGY.

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I.—HÆMATOPHAGY AS A NORMAL OCCURRENCE: THE NATURE AND ORIGIN OF BLOOD-PLATELETS AND OF THE KURLOFF-BODIES.

INTRODUCTORY.

I PROPOSE to call by the name of *hæmatophagy* the function which may be exercised by a cell of ingesting blood-cells and elements of various kinds; a cell behaving thus may be termed a *hæmatophage*; and so on. The words phagocytosis, phagocyte, etc., have become inseparably associated with the idea, merely, of the removal and destruction of some effete cell or deleterious microbe. Little or no attention has been paid hitherto to the effect or result of this process upon the devouring cell. But in regard to the ingestion of blood-cells, the question of the use made of this "solid" nutriment is so important that I do not consider the term phagocytosis, with its conventional, limited significance, is any longer suitable. The terms here proposed are to be associated with the idea of a particular kind of food taken up by the cell, and the extent to which this nutriment can be digested and utilized by the latter. My object in this paper is to indicate some of the results of the exercise, normally or in pathological conditions, by different types of cell and with different consequences, of this function of hæmatophagy.

It is very much to be regretted that, owing to the enormous expense incurred at the present day in the production of coloured plates, it is impossible to have any to illustrate my work. The illustrations suffer,

unavoidably, in being in black and white only. I am deeply indebted to Miss Rhodes for the valuable assistance she has given me; not only in making the original coloured drawings, which have been submitted for the Editor's inspection, but also in redrawing a selection of them in black and white, from which the figures here given are reproduced. I am also greatly obliged to Dr. D. J. Reid, for his kindness in taking the excellent photomicrographs. Lastly, I desire to express my grateful thanks to my friends, Dr. J. D. Thomson and Dr. Ledingham and to Professor Boycott, for their most helpful advice and instructive criticism; whatever merit the paper may have would be distinctly less but for their kind assistance. But I ought, perhaps, to add that my wish to thank these gentlemen is not to be taken as necessarily meaning that they agree with my conclusions.

Considerations of space prevent any detailed discussion of the enormous quantity of literature relating to the various subjects dealt with. I have to confine myself, therefore, to the mention of only very few papers, which are of importance in relation to the views here adopted.

Technique.

General Remarks.—The work has been entirely a cytological study, and in such the question of technique is, of course, of much importance. Far more important, however, is the interpretation of the appearances obtained; from the nature of the case, the right view or conclusion is a matter entirely of correct interpretation. And for a correct interpretation, a thorough knowledge, not only of the reliability but of the vagaries and idiosyncrasies of any particular stain employed is essential. This has certainly not been sufficiently realized in connexion with what is undoubtedly, on the whole, the most valuable staining-method for blood-elements which we possess. I refer, of course, to all those stains which may be comprehensively included in the category of Romanowsky stains—the red-blue combinations.

There are especially two points which have to be borne in mind in deciding the significance of any bodies of unknown or enigmatical character from their appearance when so stained. The first is the tendency to enormously overload with stain any cell-organ or inclusion for which some component of the stain has a strong affinity. Other stains—of "regressive" character—may also behave thus, but in their case the excess of the stain can be extracted by appropriate differentiation; in the case of Romanowsky combinations, differentiation to what would be the right extent is rarely feasible because of the liability of a particular colour (usually the red) to disappear. Thus it happens that the most minute granules are often rendered so conspicuous that they assume an air of importance which in reality they are far from possessing.

The second point is that, because a granule or a clump of granules stains red (or purplish-red) by this method, it does *not* follow that these

granules consist of chromatin and represent nuclei or true, functioning nuclear material. Their chromatinic nature must be tested by means of one of the well-known standard nuclear stains. As a protozoologist, I am astounded when I consider the quantity of work which has been published, often by competent investigators, in which such *red-staining* granules have been, *on that account* regarded as indicating the nucleus or the nuclear material of some parasite; and in which, moreover, a superstructure of a complicated life-cycle, with gemmules, gametes, spores and all manner of phases, has been built up on that fragile foundation alone! A remarkable feature is that these parasites, with their complex life-histories, are nearly always regarded as Protozoa, no account being taken of the absence, in many cases, of cytoplasm, or of any indication that these various bodies show the structure of a differentiated cell, characteristic of all Protozoa.

Two examples illustrating the above points may be given which will be within the cognizance of, or can readily be observed by anyone interested in blood-work. In an ordinary dried blood-smear, stained by Giemsa, let us say, the nuclei of the leucocytes present a beautiful picture, varying in hue (according to their type), from rich purple-red to almost brick-red. The whole nucleus is deeply stained, but rather darker masses of varying size and shape can usually be distinguished. Now this appearance of a huge red-staining nucleus by no means corresponds with the appearance of the actual chromatin present, as can be seen by a comparison of a wet-fixed film, stained, say, by iron-hæmatoxylin and differentiated in the usual manner. In such a film, the nucleus of a leucocyte does not occupy, relatively, so much space in the cell, and comparatively little of it is stained black, or greyish-black, indicating the actual amount of chromatin present. The grains of chromatin are disposed chiefly around the nuclear membrane, but a few occur also on the strands of the reticular network. All the ground-substance has practically the same tint as that of the cytoplasm, according to whatever counter-stain has been used; if none, it is neutral to greyish. (For figures illustrating the comparative appearances of the nuclei of the leucocytes of birds, when stained by these contrasting methods, see Woodcock [16].) A very large proportion, therefore, of the red-staining nuclear mass, in a Giemsa-smear, is not chromatin at all, but merely nuclear ground-substance.

The second example, on which I wish to lay particular emphasis, is the known variation in the staining character of the red corpuscles, when altered in any way. Thus, a ruptured, flattened corpuscle is often stained a strong red colour. Further, corpuscles are seen occasionally, probably effete or breaking-down, in or upon which the stain has been deposited in the form of red dots; manifestly certain fine granules are present in such cases which have a marked affinity for the red component of the stain (I am not referring, of course, to punctate basophilia). Again, under the influence of certain of the malarial parasites, red-staining granulations are regularly formed in the in ected corpuscles These vary in tint according to the type of parasite. Maurer's dots in corpuscles infected with the malignant tertian parasite, are of a much stronger red and larger, more blotchy in character than are Schüffner's dots in the case of benign tertian. *The important point to recognize is that certain products of the alteration of the substance of red blood-corpuscles may stain red by Romanowsky stains.*

It is seen, therefore, that, on the one hand, different bodies or structures, even though very closely related, may stain somewhat differently, in their normal phase and under constant conditions; and on this account they can be easily distinguished. And, on the other hand, the same organella or material may stain differently according as any slight alteration occurs in its composition; and this difference in staining character may give the object an entirely different appearance and render its recognition difficult. Hence the interpretation of the results of Giemsa-staining may appear to be most paradoxical; nevertheless, herein lies the secret both of the success and of the pitfalls attending its use.

The Giemsa-stain is invaluable for blood-work; but it *must be controlled* by a known, reliable, chromatinic stain such as hæmatoxylin—any of the various modifications will serve. Without the use of *both* these types of staining-method, I could not have gained enlightenment upon this subject of hæmatophagy.

Methods.—I have used human blood, and the blood and hæmatopoietic organs of the guinea-pig and kitten. Most of the work has been done upon the guinea-pig. Except where otherwise stated, all the preparations are from normal animals.

Most of my permanent preparations have been made according to one of the two following methods: (*a*) Smears: These were rapidly dried in air, fixed with absolute alcohol and stained with Giemsa for half an hour, the strength of the solution used being three drops to the cubic centimetre. (*b*) Films: These were made on a cover-slip and were fixed wet, i.e., by dropping them face downwards on to the fixative. As fixative, I used a special mixture of my own, devised many years ago, which I have always found excellent; I call it the S.A.A. mixture (sublimate alcohol-acetic). It differs from Schaudinn's fluid in that, instead of adding only a few drops of acetic, I add five per cent as I long ago came to the conclusion that Schaudinn's fluid does not contain enough acetic. The films were taken through the alcohols and stained by Heidenhain's iron-hæmatoxylin in the usual manner; eosin was generally used as a counter-stain. (To avoid repetition I refer subsequently to preparations made according to method (*a*) as smears; and to those made according to method (*b*) as films, except where stated otherwise.)

Now and again, however, for purposes of comparison I have stained a dried smear, after fixation with alcohol, by iron-hæmatoxylin; and on the other hand, a wet-fixed film by Giemsa.

In making smears and films from the spleen and bone-marrow, it is important not to smear the substance of the organ directly along the slide or cover-slip, but to tease up small portions thoroughly with a pair of very fine needles, in salt-citrate solution, and then make the smear or film in the usual manner. By doing this the cells are not only nicely separated, but the great majority of them are of normal appearance and intact, not ruptured or squashed-out (in the case of smears, the large megakaryocytes are at times unavoidably flattened to some extent). But in a smear made directly from an organ, frequently nothing but a "mush" results, the nuclei alone standing out from a common matrix of ground-substance representing the cytoplasm of the squashed cells.

As a specific stain for chromatin I have followed the classic method of adding dilute, acidulated methyl-green to the fresh unfixed cells or elements.

Blood-Platelets: their Nature.

Some months ago my attention was drawn by my friend, Dr. Ledingham, to a recent paper by Herwerden [6], in which the author expressed the opinion that platelets possessed the characters of differentiated cells, i.e., containing nucleus and cytoplasm. I had not previously given these elements special consideration, but as soon as I gave any thought to the question, this view struck me from what I had already observed, as being most unlikely, so that I decided to study them myself more closely, little knowing the important results to which my observations would lead me.

The usual appearance of platelets in ordinary smears is so well known that only few words are here required. When seen in the condition most nearly resembling that in which they occur normally in the blood, the platelets are generally small, spherical or ovoid bodies,[1] about one-fifth to one-fourth the diameter of a red corpuscle. But they vary in size not only in different animals but also to some extent in the blood of one and the same individual. When stained a platelet appears (fig. 1) as a clump of granules of varying size, staining usually red (the shade of colour varies according to the Romanowsky modification used), and embedded in a faintly blue-staining ground-substance; this ground-substance is sometimes hardly distinguishable apart from the granular mass. Near the edge of a smear the platelets are often collected into a clump, in which the ground-substance has coalesced so as to appear as a common matrix containing the aggregations of platelet-granules.

The first thing to ascertain was whether the red-staining granules represent chromatin or not. On applying the methyl-green test the platelets remain entirely unstained, whereas the nuclei (only) of all the leucocytes are stained a bright green. This is conclusive of the absence of chromatin in the platelets; in spite of the red colour of their conspicuous granules.

I then proceeded to examine films, mounted after varying degrees of differentiation.

In films which have been only momentarily in the iron-alum, just to cleanse the film, practically no differentiation is effected (fig. 2*a*). The red corpuscles are a dull black, more or less uniformly except in the thinner part of each, where the colour is greyish-black. The platelets are also uniformly black, or greyish-black in the case of the smallest ones; in the case of the largest, and particularly where a few are closely clumped together, the colour is an intense, bright black, almost a blue-black.

In a film which has been differentiated to the right extent as regards the leucocytes, and counterstained with eosin (fig. 2*b*), the nuclei of the leucocytes (of

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[1] However rapidly the preparation, fresh or permanent, is examined or made, a few irregular or streaky forms can nearly always be found.

any one type) all show the same organized structure, though certain will be a shade darker, and others a trifle lighter, according as the hæmatoxylin has been rather less or rather more extracted.[1] On the other hand in the case both of the corpuscles and of the platelets, the uniformly diffused stain comes out entirely, but in a patchy manner. That is to say, in some cases, part of the corpuscle or platelet will still retain some of the stain, while the rest is free from it. In the case of the homogeneous substance of the corpuscle, the limit of the still-staining portion is ill-defined and shades gradually away. In the case of the finely granular substance of the platelet, the stained portion is rather better defined, but still patchy. And, of course, as more and more of the black stain is lost, the contrasting pink of the counterstain is more apparent.

Hence, while some large platelets, or compact clumps, are still black, others show little patches of varying size still black, the rest being pink; while yet others are entirely pink.

Thus in the platelet, as in the corpuscle, there is *no* definitely organized internal body, retaining its shape and structure, only becoming gradually fainter as the stain is extracted.

Impressed by the similarity between the corpuscles and the platelets, as regards the manner in which the black stain can be readily removed *in toto*, I dehæmoglobinized some smears, by laking the blood with water in the usual way, and then fixed them with absolute alcohol and stained with iron-hæmatoxylin. These smears, too, were merely rinsed for a second or two, with the alum, to clear away the excess of stain, so that they had no differentiation (fig. 3). No traces of the corpuscles are present, with the occasional exception of a faint ring, denoting the "skin." The nuclei of the leucocytes are wholly dense black, showing no structural differentiation. Their cytoplasm is of the customary pale, neutral tint. The platelets appear as uniformly pale bodies, their substance being very finely granular. There is no trace of the intense black stain about them. The substance which stains thus in ordinary films has been dissolved away in the laking, just as is the case with the hæmoglobin of the red corpuscles. Only, while the entire substance of the corpuscle has vanished, the substance of the platelet is left.

What could this black-staining substance be, which is present to a considerable extent in the platelet? It appeared to me most likely that it was some iron-compound, loosely associated with the substance of the platelet, but not intimately united with it, as is the iron in chromatin; a compound which was readily soluble in water, as is hæmoglobin. The question arose whether hæmatoxylin-stains (e.g., iron-hæmatoxylin, or Delafield's) could be regarded as indicating "masked" iron-compounds, or certain of them, by staining the elements containing such; in other words, whether it is on account of the presence of combined iron, in certain forms, that such elements stain by this method. MacCallum [7], dealing with iron-compounds in cells, says that "the hæma-

[1] It must be noted that, in any film, the degree of differentiation is rarely uniform throughout. Parts where the film is thicker tend to be less differentiated than others where it is thinner. Hence different individual cells or elements of the same type show slightly varying degrees in the amount of extraction of the stain.

toxylin stain" (this worker used Erhlich's modification) "in the chromatin is always found to correspond in intensity, in the object stained and in the general distribution of the stain, with the blue reaction obtained in the other sections" (i.e., the Prussian-blue reaction used as the microchemical test). Similarly, the hæmoglobin of the red corpuscles is stained intensely, as already noted (and also, it may be added, when Delafield's modification is used). I was inclined, therefore, to answer this question provisionally in the affirmative, and also to regard the black-staining substance of the platelet as representing some iron-compound. I may mention here, that following Professor Boycott's kind advice, I have been able to demonstrate the presence of iron to a considerable extent in "platelet-cytoplasm," i.e., the cytoplasm from which the platelets are formed, by means of a definite microchemical test (*vide* below, p. 334).

I had therefore, a certain amount of light upon the nature of blood-platelets. They are not complete cells, because they possess nothing of the nature of a nucleus; they do not consist, at any rate, of unaltered nuclear material, because they have no chromatin. Platelets are bodies consisting of protoplasm—organized material, and not merely organic (e.g. some proteid), or it would be, in all probability, homogeneous—with which is associated a relatively considerable amount of intensely staining substance which I regarded provisionally as being some iron-compound.

Blood-Platelets: their Origin.

The latest work of which I knew at this stage, one dealing with both platelets and thrombocytes, was the valuable résumé of Werzberg [14]. This author sums up the question of origin in these words: "As regards true platelets it is now well-established (apart from the work of Wright) that they are derived from the contents of the reds." By the "reds" he means, it is to be gathered, the immature reds still containing the nucleus; and he does not definitely commit himself further. The only paper advocating the origin of the platelets from the megakaryocytes, of which I was then aware, was one of Wright's first papers [18], namely, the reference given by Werzberg. And the chief reason why I paid very little attention to the view of this author at the time was because of my opinion that the platelets contained a considerable amount of iron. MacCallum says (loc. cit.): "the presence of assimilated iron, apart from its occurrence in hæmoglobin and hæmatin, is an exceptional feature in the cytoplasm of the cells of the higher forms of animal life." The only exceptions, apart from hæmatoblasts, are yolk-containing cells, etc., and ferment-forming gland-cells. Hence I did not see at this stage, how platelets could possibly be derived from any kind of white blood-cell.

The information I had gained suggested, therefore, some connexion with the red blood-cell elements as being most probable. I was very doubtful about an origin directly from mature corpuscles, because it seemed to me that one can hardly regard the substance of the latter as being (any longer)

organized cytoplasm.[1] Otherwise when the hæmoglobin is dissolved away there ought to be some protoplasmic ground-substance left behind; whereas there are, at most, traces of a delicate enclosing "skin" or membrane.

For a time I was inclined to think that the platelets might be derived directly from the nuclei of the immature reds by their gradual breaking-up; whether while yet inside the cell or after extrusion therefrom, had to be ascertained. MacCallum (loc. cit.) has shown that the hæmoglobin is developed from the chromatin of the nucleus of the corpuscle-forming cells. And I thought it was quite possible that after the formation of the hæmoglobin was completed, the nucleus might continue to undergo alteration, with resulting disappearance of the remaining chromatin, separation of the iron-substance and eventual fragmentation into platelets. Holding this view tentatively, I proceeded to the study of the hæmatopoietic organs.

I will not weary readers by describing the history of this period. Suffice it to say that I went from bone-marrow to spleen and from spleen back again to bone-marrow in a search for indications. Both in the bone-marrow and spleen I found an abundance of immature reds (normoblasts), numerous free, extruded nuclei of these (fig. 5*b*), and here and there a nucleus in the act of being extruded; this act, however, is most difficult to catch. I saw no signs of the gradual break-up or dissolution of the nucleus in the developing corpuscle, and it was clear that the vast majority[2] at all events, of the nuclei are extruded intact and still possess chromatin. Neither could I see any indications of the alteration or fragmentation of these free nuclei; and all of them are very much larger than any individual platelet.

For a time, therefore, I was at a loss and incidentally turned to the study of the Kurloff-bodies, whose appearance in the smears I had been examining had intrigued me greatly. As soon as I saw these bodies in *films* I not only realized their nature and origin (see below), but was put on the right track in regard to the origin of the platelets. I at once concentrated my attention upon the macrophages (the large mononuclears, or "transitionals," and the megakaryocytes) and found that *the platelets represent certain products of the digestion of blood-cells and elements in*

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[1] As they are certainly not complete cells, I restrict myself to the use of the words red corpuscle or corpuscle, to distinguish them from complete blood-cells, such as the leucocytes.

[2] In smears of the blood of a purpuric guinea-pig, kindly given me by Dr. Bedson, here and there corpuscles (with hæmoglobin), can be found containing still small nuclear fragments (so-called Jolly bodies); and immature corpuscles possessing a nucleus which is apparently undergoing fragmentation in situ. It is possible that these small bodies may ultimately lose their chromatin and become platelets; but that this method of formation, if it occurs normally, only does so to a negligible extent, I am convinced by my study of the organs of normal animals.

the cytoplasm of the macrophages, together with a small portion of the disintegrating cytoplasm itself, abstricted from the cell.

The Small Macrophages (large Mononuclears and "Transitionals").

As there are rarely more than two or three blood-cell elements undergoing digestion at once in a large mononuclear, and the digestive vacuoles are nearly always separate from each other, the progress of the alteration and the concurrent appearance of the platelet-granules can be seen here much more distinctly than in the case of the megakaryocytes, in which the bulk of the cytoplasm is often in the condition of "platelet-cytoplasm." And although the large mononuclears perform their work individually in a much more modest and less demonstrative fashion, there can be no doubt, I think, that in the aggregate they must produce a considerable quantity of platelets because of their numbers.

The process usually known as "phagocytosis" goes on normally to an enormous extent in the spleen and bone-marrow, particularly of course in the former organ. In the circulating blood, macrophages containing ingested red corpuscles are rarely, if ever, met with normally. But in the case of some of my smears and films, especially of spleen-pulp, one has only to allow one's eye to traverse a few fields to see dozens of these cells, containing usually one to three, or even occasionally four or five corpuscles (cf. figs. 4*a* and 4*b*). Doubtless, the extent to which the process is going on varies slightly from time to time, according to the number of corpuscles becoming at the moment effete; for it may be inferred that normally[1] only the worn-out corpuscles will be eaten. In addition to the corpuscles, however, these cells take up the free nuclei of the immature reds; at times a cell can be seen containing both these kinds of element (fig. 4*c*). Old lymphocytes and polymorphs are very rarely eaten by the large mononuclears, but I have occasionally seen what I took to be a lymphocyte-nucleus in course of digestion. The red corpuscles must be taken up with extreme rapidity because I have never caught a large mononuclear which seemed to be in the act of engulfing one.

As regards the nature or meaning of the changes which occur during digestion, I am, unfortunately, unable to determine them. Such work comes within the province of biochemistry. All that I can do is to give some idea of the microscopical changes to be observed. It is highly probable, of course, that

[1] In this connexion the record of a fatal case of anæmia, by M. Rowley [11] is of great interest. Here phagocytosis of the corpuscles, "healthy" ones included, it would seem, was occurring abundantly in the general circulation. While the large mononuclears were chiefly engaged in this destruction, other leucocytes (polymorphs) also took part. It is to be noted that the authoress remarks incidentally, that the blood-platelets were enormously increased in number; and she observed them in the act of being cut off from the cytoplasm of the devouring cells.

some ferment or enzyme secreted by the nucleus is largely concerned in the process; we know this to be the case with many Protozoa, where the course of intracellular digestion has been studied. It is significant that an ingested corpuscle, in the digestive vacuole (often "virtual" rather than apparent) usually comes to lie very close to the nucleus, which, often, indeed, partially surrounds it.

The first change is that the hæmoglobin is rendered colourless—bleached, as it were—so that the corpuscle appears as a practically colourless vacuole. Next, in close association with the corpuscular substance, as a result of further change, the characteristic red granules are formed. At first these are very fine and minute, and stain only faintly red; often the appearance is more that of a very finely granular, pale red-staining substance. Subsequently the granules become discrete, much more prominent and stain deeper (cf. different stages in fig. 4, right-hand side). The vacuolar appearance is gradually lost as the assimilable products of the digestion are incorporated into the general cytoplasm. Unlike what is the case in the megakaryocytes, the red granules do not generally become uniformly dispersed throughout the cytoplasm, but tend to remain in more or less distinct clumps. The granules represent apparently the unassimilable residue of the digestion, and as such are more of the nature of metaplastic products—non-vital and passive—than an incorporated, essential constituent of the cytoplasm.

A most important point to note is that I have never seen the slightest indication of the formation of pigment-grains in connexion with this digestion of the corpuscular substance. Iron-containing pigment is, of course, not always formed in connexion with the true digestion of hæmoglobin by cells, e.g., parasites. Thus, while the malarial parasites produce pigment, *Piroplasma*, etc., the Hæmogregarines do not. Again, in cases where a parasite devours whole corpuscles, *Entamœba histolytica* and *Balantidium* do not form pigment, whereas a blood-eating Ciliate from a whale, recently described by Woodcock and Lodge [17], under the name of *Hæmatophagus*, produces masses of pigment. Now, in both types of cases, i.e., the malarial parasites and *Hæmatophagus*, the production of the pigment-grains, *in situ*, at first fine and then becoming larger and more conspicuous, is always seen without the slightest difficulty. Nothing of the kind occurs during the digestion of the red corpuscles by large mononuclears.

Two conclusions follow, I consider, from this point. First, that the *iron of the hæmoglobin is retained in some form in the cytoplasm of the macrophage*; (and equally, of course, that derived from the chromatin of ingested nuclei). Secondly, that the grains and clumps of yellow (ferruginous) pigment, commonly occurring in the spleen, are *not* derived from the digestion of the red corpuscles, i.e., are not a direct product of "phagocytosis." How these pigment-masses result I cannot say; I can suggest that, in large part, they may be formed from the ultimate disintegration of the platelets themselves, and also perhaps, when red corpuscles or other elements disintegrate in the plasma.

It is noteworthy how rarely large mononuclears can be found containing any pigment, even though the smear contains large numbers of cells with corpuscles undergoing digestion. Contrast this scarcity with the abundance of pigment-containing macrophages in a malarial spleen. I consider that, far from getting rid of the iron (in the form of pigment), the mononuclears require this substance for the "platelet-cytoplasm," and it may be on this account, owing to the large

destruction of red corpuscles by the parasites, that these cells eat the pigment in cases of malaria.

To complete the consideration of the normal course of the digestion, after this digression, it may be added that, in the case of ingested nuclei, there is, of course, no hæmoglobin to consider. The nucleus seems at first to become denser and to stain, if anything, more deeply than it does normally. But as it undergoes digestion, and its structure becomes altered, it appears much looser in character and stains more lightly. Ultimately it becomes entirely granular, the granules staining bright red. It is, in fact, at times difficult to be sure whether a granular mass results from nuclear or corpuscular digestion; the granules in the former case tend to be rather more prominent.

The further behaviour of the macrophages and the actual production of platelets is better dealt with in relation to the megakaryocytes, where the process is on a much more comprehensive scale.

The Large Macrophages (Megakaryocytes).

The megakaryocytes are most remarkable cells. Though they are well-known I make no apology for considering them here in some detail.

According to Guieysse-Pelletier [5], a megakaryocyte begins life as an endothelial cell; this author has noted their origin in this manner in the case of white mice. A megakaryocyte, when quite young, is indistinguishable from a cell of large mononuclear or transitional type; and such cells, too, are regarded as being of endothelial origin. In my films a perfect series of transitions can be found between cells of large mononuclear character, with a lobed or indented nucleus, and huge, full-grown megakaryocytes (fig. 5). The megakaryocytes occur predominantly in the bone-marrow, but also in the spleen. But in my preparations of the latter organ, their occurrence is much more variable than in those of bone-marrow. This may be due to different preparations having been made, unthinkingly, from different parts of the spleen.

The outstanding features in the life-history of this form of cell is that cell-division, *as a whole*, is very largely inhibited, or in abeyance. The cells doubtless divide at times; but normally I should say only very exceptionally, unless when quite young. The end of a full-grown individual is not division and production of an entirely new cell-series, but death and disintegration.

The growth of a megakaryocyte is characterized by the continual increase in the amount of nuclear material. As the cell elaborates more and more nuclear substance (including chromatin), the nucleus becomes massive and lobed, and often undergoes division, unaccompanied by cytoplasmic partition. Thus a multinucleate condition results. In no other way can the many nuclei which several of these large cells possess, be adequately explained. It is quite likely, though this is only inference from the constrictions often to be observed in a nuclear mass, that the mode of division when unaccompanied by cytoplasmic division, is little more than that of simple fission. The separate nuclei in any one individual

are generally approximately equal in size and of the same slightly ovoid shape; they all have, moreover, an identical appearance, staining character, etc., which is different from that of the nuclei of the ingested cells.[1]

The megakaryocytes eat chiefly leucocytes (polymorphs and lymphocytes), and the free nuclei of the normoblasts (figs. 5*c* and 5*e*); but in addition, red corpuscles are sometimes eaten (fig. 6). This hæmatophagic behaviour on the part of the megakaryocytes is always taking place, normally, in a quiet way. In any smear of bone-marrow or spleen, some of these huge cells can be found containing ingested cells, but the occurrence does not strike the eye in such a marked manner, of course, as does the "phagocytosis" by the large mononuclears. For one thing, there are not nearly so many megakaryocytes; and for another, the red cell-elements are mostly disposed of by the mononuclears, and, of course, these vastly predominate in number over the leucocytes. To show, however, the degree to which hæmatophagy may take place in the case of the megakaryocytes, on occasion, I have been most kindly allowed by Dr. Ledingham to have the photos taken which are reproduced in figs. 7 and 8, from the bone-marrow of a purpuric guinea-pig. (For description, see explanation of the figures.)

I have never seen any signs of pigment-grains in a megakaryocyte.

It is only from films that one can realize how extremely amœboid these great cells are. Many of them are caught showing blunt pseudopodial outgrowths of varying size and shape (fig. 5*c*). A megakaryocyte, in fact, resembles in several respects a large predatory Amœba! But herein lies an essential difference, namely, that the megakaryocyte is periodically engaged in *losing* its pseudopodia, whether large or small. How far this is an active (voluntary) process, or how far involuntary—a simple moving away of the cell, leaving eventually a portion of its cytoplasm broken off behind it—I am unable to say. I have an example of a megakaryocyte which shows a large pseudopodium with a pronounced constriction at its basal end, appearing as if it were in the act of being abstricted. These separated portions of megakaryocyte-cytoplasm, large and small, can be easily recognized in the smears.

Owing to this periodic loss of cytoplasm, there is at times considerable variation in the nucleo-cytoplasmic relation obtaining in these cells. While in some, there is an ample quantity of cytoplasm, in others, doubtless corresponding to a stage in the cell-life following upon the abstriction of large portions of platelet-cytoplasm, there is very little. At such a time, the cell consists mainly of this huge nuclear complex.

Further, at different periods, the quantity of chromatin contained in the

[1] It is a ridiculous hypothesis to suppose, as is the opinion of Guieysse-Pelletier (loc. cit.) that where there are multiple nuclei, the additional ones are actually the ingested nuclei. It is impossible to imagine that a foreign nucleus could be incorporated bodily and become a constituent, functioning organella of the strange cell. No doubt the extra nuclear material is built up from the assimilated substances derived from the nuclear food—but indirectly.

nuclei varies greatly; sometimes the nuclei are intensely stained; at other times (in the same film) they are so poor in chromatin that they are almost pallid (there is no question here of this great difference being due merely to varying differentiation). It is most instructive to see a cell with large, pale nuclei and little cytoplasm, in this extreme "hunger-phase," containing several recently ingested cells or nuclei, from the assimilated products of whose digestion it will build up its protoplasm anew (fig. 5*c*).

Practically the whole of the cytoplasm is potential "platelet-cytoplasm." The platelets result either from the break-up of the abstricted cytoplasmic fragments; or, especially in the case of older cells, approaching a worn-out condition, they may be separated off directly from the periphery of the cytoplasm, which has then a particularly frayed-out appearance (fig. 5*d*). The realization of the essential platelet-character of this cytoplasm is readily obtained from smears (or films stained by Giemsa).[1] The characteristic, small, red-staining granules are often distributed uniformly throughout the dense, blue-staining cytoplasm. Where the granules are very fine, the cytoplasm, for this reason, acquires almost a mauve tint. Near the periphery, and especially in a lobed portion, or pseudopodium, the granules become definitely aggregated into clumps (fig. 9). The larger, free masses also show this well-marked distinction. Even in small abstricted portions, containing only aggregations of granules, the cytoplasm is still more definite and more deeply blue-staining than is the case where clumps of platelets are seen in the blood. As the small masses break up into separate platelets, the cytoplasm probably breaks down still further, to become of the delicate and faint-staining character associated with the discrete platelets.

The end-stage in the life of a megakaryocyte occurs commonly in some of my films (fig. 5*f*). The large, extremely pallid nuclei are scattered about, two or three often being still connected together by a fine thread (nuclear membrane?); their cytoplasm has practically vanished, but there are usually a few stray tags still attached, some of which are almost resolved into platelets.

The behaviour of the cytoplasm of the large mononuclears is essentially comparable with that just described in the case of the megakaryocytes, only the process goes on in miniature. Small abstricted portions of the cytoplasm of these cells, containing aggregations of granules, occur in numbers (fig. 4*d*); but each such small mass will only break down usually into a few platelets.

I had worked out this whole question of the nature and origin of platelets—many of the drawings being actually made—before I ascertained that considerable confirmation of Wright's work had been already brought forward. Only a few

[1] As in so many other cases, the granules are unduly conspicuous after Giemsa; they are not at all prominent, apart from the general granular character of the cytoplasm, in films and have no particular affinity for hæmatoxylin (cf. fig. 5 with fig. 9).

days before beginning to write this article, I was steadily searching through the later volumes of the *Folia hæmatologica* with a view to finding what was the latest of Schilling-Torgau's ever-changing opinions upon the nature of the Kurloff-bodies, when I came across a paper by Hal Downey [4] upon the origin of platelets from megakaryocytes. Many of Downey's figures showing the abstriction of platelet-cytoplasm present just the same appearance that I have found in my preparations. And had I known at the commencement of my work of Downey's paper, I should certainly have studied the macrophages earlier than I did. As Downey somewhat pathetically remarks: "Wright's work has not been generally accepted, especially in Europe."

All credit, therefore, must be given to Wright and his followers for having determined the immediate origin of the platelets from megakaryocytes. Their view is correct, so far as it goes, but they have not taken into account what is, I consider, the most important factor in the process.

In the first place, Downey describes also the pinching-off of small buds of cytoplasm both from large mononuclears and from lymphocytes, but is of the opinion that neither of these types of cell gives rise to platelets. He is quite right as regards the lymphocytes; as will be seen in a moment, in connexion with the Kurloff-bodies, these leucocytes (which are, rather, microphages) *cannot* (normally) form platelets even if they become hæmatophagic. But the large mononuclears certainly can do so; it was from these particularly that I learnt the secret of the real origin. The mononuclears, and these only, are of essentially similar origin and character to the megakaryocytes. Both these macrophages not only feed in the same manner, *but are able to digest and assimilate the blood-elements taken up*; that is the important point.

And this is the point which Downey has entirely missed; he considers that the granules of the platelets are actually extruded from the nucleus, i.e., that they represent directly some nuclear material. I have not seen the slightest evidence of such extrusion in my preparations and this view is, I think, from what has been shown above, untenable. Some ferment or enzyme is doubtless secreted by the nucleus; but the granules are a by-product of the interaction between this ferment and the food-material undergoing digestion.

To sum up: *platelets are a direct result of hæmatophagy*; in my opinion, if the macrophages (large mononuclears and megakaryocytes) did not eat and digest blood-elements of all kinds there would be no platelets.

Note on the Determination of the Presence of "Masked" Iron, by means of the Prussian-blue Reaction.

After considerable difficulty I have succeeded in obtaining definite indications of the occurrence of iron, on the application of this microchemical test. Adopting first MacCallum's method of prior treatment with four per cent sulphuric acid-alcohol, I met with no success, either as regards platelet-cytoplasm or (it may be here mentioned) the Kurloff-bodies. Only the chromatin of the nuclei reacted

to the application of potassium ferrocyanide+dil. HCl. It was evident, therefore, that the form in which the iron is combined in the cases under consideration, is almost as difficult to break down as it is in hæmoglobin, although it is a different compound. However I came across a paper by Brown [3] on the liberation of iron from hæmoglobin and allied compounds by means of hydrogen peroxide and after many trials I succeeded in getting this oxidation-method to work. Films and smears were placed in a fifteen per cent solution of this reagent (Merck's "Perhydrol," diluted with an equal volume of distilled water—the ordinary weak solution gives no result) for two, or even three days. They were then well washed with water and placed for five or six minutes in the ferrocyanide-HCl mixture. The preparations were mounted without being "stained" in the ordinary way, because it must be remembered that the colour obtained by the reaction is much more delicate, as a whole, than the strong effects produced by staining with, say, iron-hæmatoxylin and eosin.

The blue is relatively deepest and strongest, of course, in the cells which are largest and of greatest depth (in wet-fixed films). Thus, in the megakaryocytes, the blue of the cytoplasm stands out conspicuously from the surrounding cells. The peripheral nuclear ring (or rings), which contains most of the chromatin, is also strong blue, but the central part of the nucleus is distinctly pale, i.e., there is little or no iron in the nuclear sap. The cytoplasm of the large mononuclears is also definitely blue, but lighter than in the much larger megakaryocytes. On the other hand, the cytoplasm of the lymphocytes and polymorphonuclears is quite pale and, indeed, in these "unstained" preparations can often scarcely be made out at all. The red corpuscles themselves are a light blue, rather a greenish-blue; I should say the iron in the hæmoglobin is the most difficult to liberate completely of any of the iron compounds considered. Owing to the delicacy of the reaction, the small, individual platelets are difficult to detect in a film. In a dried smear, however, especially in the "tails," these can readily be found and are seen to have a distinct coloration, comparable to that of the chromatin of the nuclei. (For some reason, in a dried smear, the chromatin does not react so markedly as it does in a film.) But, on the other hand, the cytoplasm of the polymorphs and lymphocytes is entirely colourless.

The Kurloff-Bodies.

The next question is, apart from the macrophages are any other of the blood-cells ever normally hæmatophagic? There is an instance (at present regarded as a very exceptional instance) of another type of blood-cell, namely a lymphocyte, which behaves in this manner normally—that is to say, customarily[1].

[1] I think that bodies comparable in origin and nature to Kurloff-bodies may yet be found to occur in other cases. A comprehensive paper by Werzberg [15] on the comparative hæmocytology of cold-blooded vertebrates, contains certain figures of inclusions in lymphocytes which, to my mind, are most suggestive of Kurloff-bodies. Again, Balfour, in an important paper [2] which is a mine of information in regard to the explanation of the various puzzles and fallacies liable to be met with in blood-examinations, states that he has encountered similar bodies in the "mononuclears" of rats and fowls. One may certainly expect to find them occasionally in other rodents, but they will be in lymphocytes, not in true large mononuclears.

The enigmatical bodies known as the Kurloff-bodies, which occur very frequently in the lymphocytes (usually the large ones) of the guinea-pig, have been the subject of many papers. The two or three most recent, of which I am aware, are those by Pappenheim [10], Schilling-Torgau [12] and Schulhof [13], to which readers are referred for consideration of the earlier work. From these papers, it is clear that there are at present two opposed views as to the nature of the Kurloff-bodies. (1) They are either, as a whole, parasites, or else they indicate very minute parasites enveloped by a secretion-product of the cell, or by extruded nucleolar material, the actual parasites not being discernable; in the latter case, they are regarded as Chlamydozoa of some kind, surrounded by a "mantle." (2) They are cell-inclusions, consisting only of secretion-products of the cell itself —"autogenous physiological formations." Schilling-Torgau's latest opinion seems to be that they are of "archoplasmic" nature.[1] All the authors named hold the second view and have now, at all events, discarded the parasitic hypothesis.

Only brief reference is necessary to the usual well-known appearance of these bodies, as seen in an ordinary smear (fig. 10). Within a clear space or vacuole, in the cytoplasm of the cell, elements occur of very varied form and size, which stain homogeneously an intense red. There may be only a single fairly large round mass, or two or three smaller ones; or there may be numerous inclusions in the form of curving threads or rods, short or long; or, finally, there may be merely a number of granules of varying size. The size of the vacuole itself, i.e., of the whole Kurloff-body, also varies greatly and without reference to the condition of the contained elements.

These bodies are to be found both in the circulating blood and also in the bone-marrow; in some animals they are numerous, whereas in others they are scanty. But they always occur most abundantly, by far, in the spleen. And as soon as I examined *films* of this organ, I found that most of the Kurloff-bodies had *an appearance identical with that of the red corpuscles.*

In a preparation differentiated to the right extent for the leucocyte-nuclei, most of the corpuscles (fig. 11*a*) still contain a good deal of the iron-hæmatoxylin not yet extracted from the central part; a smaller or larger zone around the periphery (according as more or less hæmatoxylin is left in the middle) is stained red with the eosin. From other corpuscles the black stain has been nearly all extracted, only a few little patches, especially in crenated corpuscles, being left in. Scarcely any corpuscles happen to be entirely red. Most of the spherical Kurloff-bodies are stained as a whole in just the same manner (fig. 11). They

[1] Balfour (loc. cit.) remarks that "Schilling's work goes to show that they" (i.e., the Kurloff-bodies) "are merely phagocyted structures." Balfour has grasped the truth there, but it is not quite clear to me whether this is his own opinion of Schilling's (Schilling-Torgau's) work, or whether he means that Schilling himself has come to this conclusion. I think the former must be the case, because I cannot find any reference by Schilling himself to such a view. And in the paper cited in the text, in which he tabulates his annual changes of view for several years past, there is no mention of such a possible explanation of these bodies.

FIG. 1.

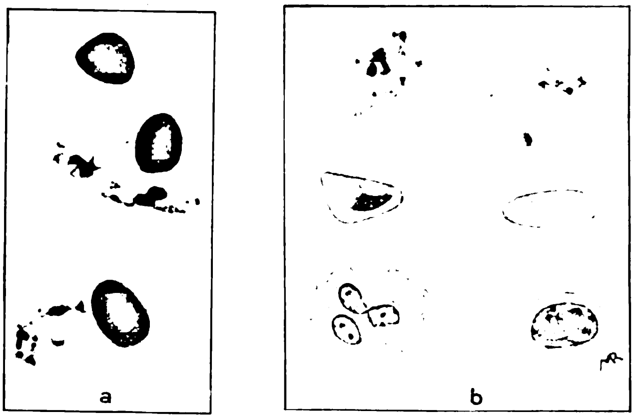

FIG. 2.

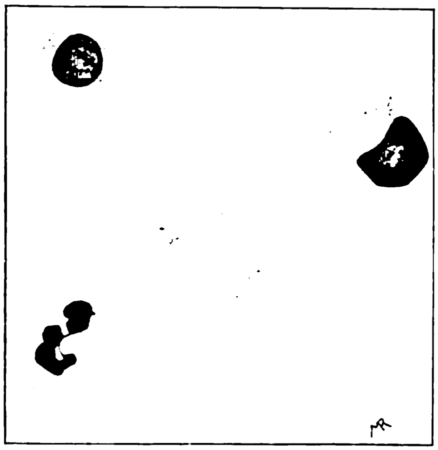

FIG. 3.

To illustrate "An Introduction to the Study of Hæmatophagy," by H. M. WOODCOCK, D.Sc.Lond.

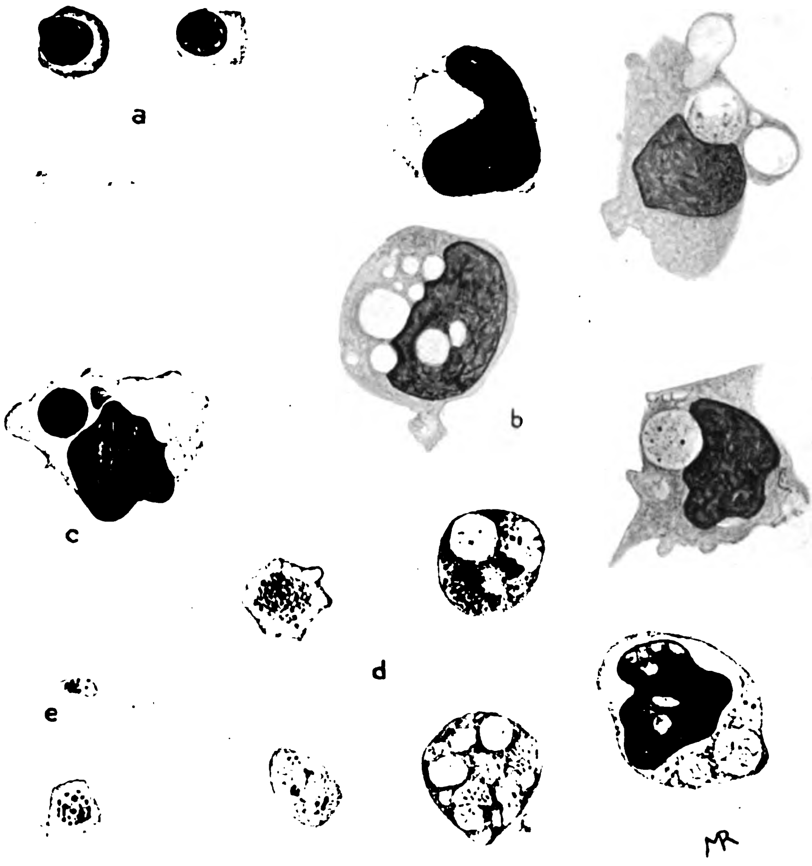

FIG. 4.

To illustrate "An Introduction to the Study of Hæmatophagy," by H. M. WOODCOCK, D.Sc.Lond.

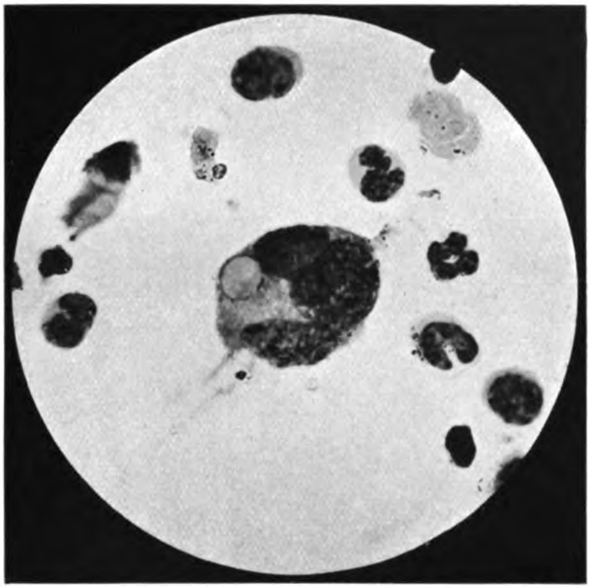

FIG. 6.

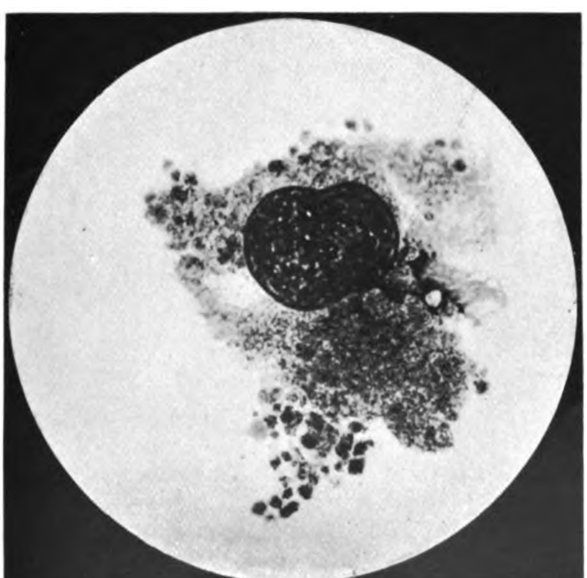

FIG. 9.

To illustrate "An Introduction to the Study of Hæmatophagy," by H. M. WOODCOCK, D.Sc.Lond.

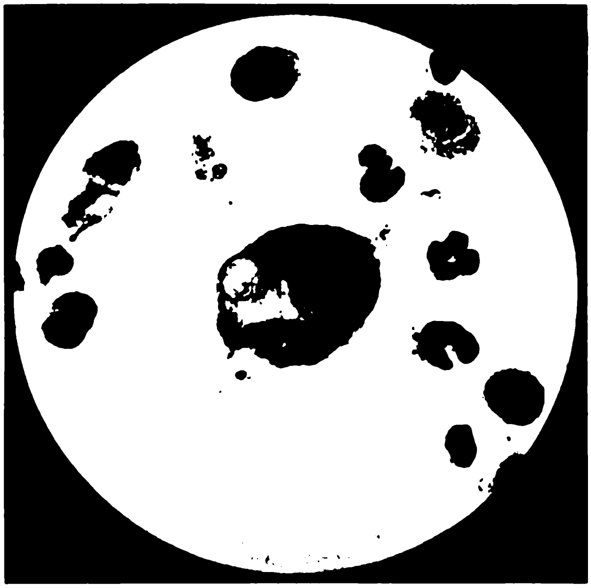

FIG. 6.

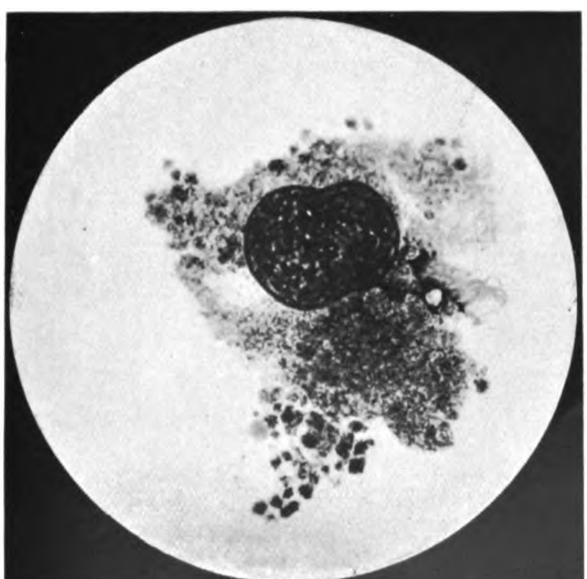

FIG. 9.

To illustrate "An Introduction to the Study of Hæmatophagy," by H. M. WOODCOCK, D.Sc.Lond.

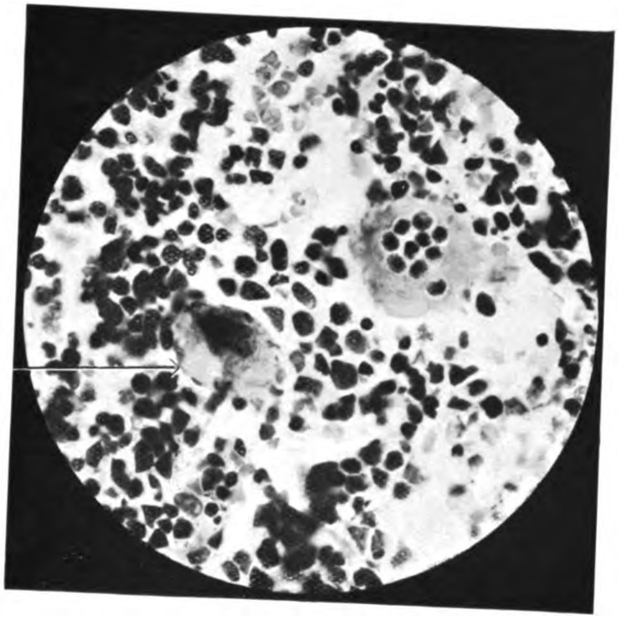

FIG. 7.

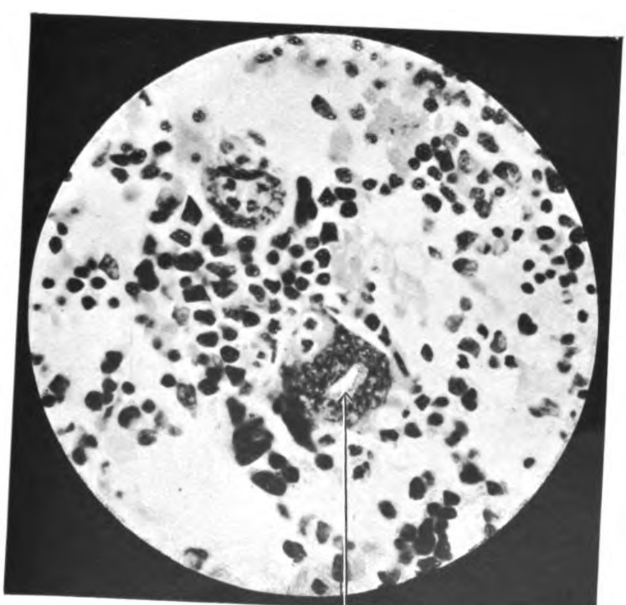

FIG. 8.

To illustrate "An Introduction to the Study of Hæmatophagy," by H. M. WOODCOCK, D.Sc.Lond.

FIG. 10.

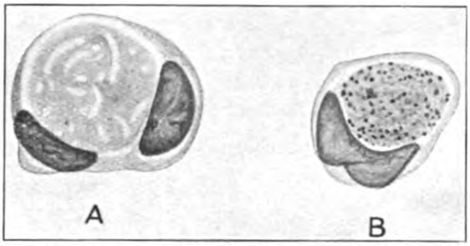

FIG. 12.

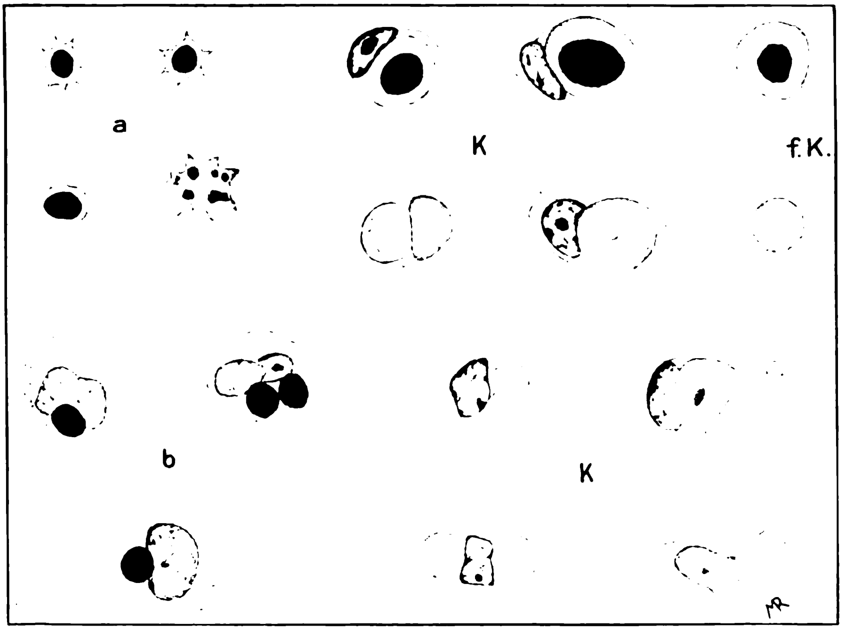

FIG. 11.

To illustrate "An Introduction to the Study of Hæmatophagy," by H. M. WOODCOCK, D.Sc.Lond.

show a central part, larger or smaller, stained black,[1] the periphery of this area being often fuzzy or indefinite; and an outer zone stained red. In all cases, equally as in the case of the corpuscles, some homogeneous substance is being stained. Varying degrees in the loss of the black stain are seen, until some of the larger Kurloff-bodies are left with none. In the case of small bodies, those about the size of a red corpuscle, the black stain has invariably gone. Sometimes there may be two Kurloff-bodies in one lymphocyte on opposite sides of the nucleus.

Here and there in the large bodies from which all the black stain has been extracted, pale unstained elements can be seen of varying size and shape.

What an entirely different picture is here presented from that found in ordinary smears. Its manifest explanation is this. The large clear space in which the intense red-staining inclusions occur, in smears, is in reality filled with a uniform substance which stains by iron-hæmatoxylin or eosin exactly as does the hæmoglobin of the corpuscles. The substance is most probably liquid in state, because of the spherical shape of the Kurloff-bodies in films. On the other hand the remarkable inclusions themselves have little or no affinity for either hæmatoxylin or eosin. (At times there is the faintest indication of greyish-black around their margin, cf. under the Negri-bodies, to be dealt with in the December number.) Kurloff-bodies occurring free (fig. 11) can indeed only be distinguished from corpuscles when they are of larger size than the latter (unless the pale inclusions happen to show through the eosin).

Why do not the homogeneous contents of the "vacuole" which provide so striking a feature in the films, appear stained in ordinary Giemsa-smears? I think the reason is because the contents are no longer there. In the first place many of the Kurloff-bodies are ruptured (or rather the delicate cytoplasmic envelope enclosing them is) as the smear dries; and frequently some of the inclusions are seen partly or altogether outside the "vacuole." And the contents have most probably been washed away during the process of staining, etc. In wet-fixation, on the other hand, there is no drying and no rupture, and the cytoplasmic envelope is preserved intact.

A film stained by Giemsa is most instructive, and provides convincing evidence of the relation between the Kurloff-bodies and red corpuscles, if the particular film contains very early stages in the formation of the bodies (a spleen-film is most likely for this purpose). In such a case the contents of certain of the "vacuoles"—varying in size according as they represent one, two or perhaps three, recently ingested corpuscles—are still distinctly yellow in tint, with possibly the merest trace of pink added —*unmistakably of corpuscular origin.* In others the contents are definitely a pale rose colour; this appearance indicates a stage in the progress of the alteration. Lastly, in the fully formed Kurloff-bodies the whole contents

[1] It must be emphasized that there is no question of this black-staining area representing any nuclear body, any more than in the case of the corpuscles; it is entirely a question of the degree of extraction of the stain from the thicker central part.

are stained uniformly a strong, bright, rose-colour; the contained inclusions, also here quite colourless (unstained), are usually though not always (cf. fig. 12*a*) hidden. Thus a perfectly corresponding picture is obtained to that seen in films stained in the usual manner.

I have obtained this rose-staining in an ordinary *dried* smear in the following manner: the smear is made and left aside for some days unfixed and untreated in any way, and then fixed with absolute alcohol and stained by Giemsa. In one case I divided a smear, freshly made, into two halves by a grease-pencil mark; I fixed and stained one half at once in the usual manner, and the other not until two or three weeks later. The Kurloff-bodies in the two halves appear entirely different and would not be taken for the same structure. In the one half nothing but the strongly stained inclusions of the customary varied form; in the other the whole body a mass of rose-colour. Long exposure to the air must in some way alter the composition of the originally liquid contents of the "vacuole," "fixing" this substance as it were, and preventing it from being dissolved or washed away by the alcohol or water subsequently.

An interesting point (in relation to the Negri-bodies) is that, in smears thus left awhile the Kurloff-bodies may show, in addition to the rose-coloured mass, numerous granulations (fig. 12*b*); these are dark red in colour and tend to be more superficial in disposition, i.e., near the periphery. They do not represent, it must be noted, the granular inclusions in the "vacuole" of some Kurloff-bodies as seen in an ordinary smear. These granules are not noticeable in *films* however stained.

I have been fortunate enough to find what I regard as definite indications of the very recent ingestion of a corpuscle and of a portion of a corpuscle (at any rate smaller than the average size)[1] by a short pseudopodium of a lymphocyte. In one instance there is already a large Kurloff-body in the lymphocyte, and this additional small quantity of corpuscular material would undoubtedly have been added to it (fig. 11). In this manner the Kurloff-bodies may increase in size. The cell continues to take up corpuscles and as a rule, instead of remaining separate, *as is nearly always the case with the corresponding ingestion in large mononuclears*, all go to form one large body. In one instance a lymphocyte is seen which has ingested two corpuscles separately and the two masses are on the point of uniting into one.

In the other case a lymphocyte has just ingested a corpuscle by means of a short pseudopodium. The altering corpuscle is still some little distance from the nucleus, which almost invariably becomes ultimately partially wrapped round the Kurloff-body.

Only red corpuscles are eaten. I have never seen the slightest indication of an ingested nucleus with its recognizable structure, and of course its chromatin staining in the usual manner.

It may be asked, how do I know that I am dealing invariably with lymphocytes and not with large mononuclears which have just ingested a corpuscle in the usual course? Firstly, by the characters of the nucleus and cytoplasm, the

[1] Small corpuscles, or what are portions of broken-down corpuscles, occur commonly in the spleen preparations.

distinctions between which in the two cases, are shown particularly well in my films made in the customary way. Especially the greater quantity and the granular pink-staining character of the cytoplasm of the large mononuclears contrasts definitely with the small quantity (usually) and the pale staining, fine character of the cytoplasm of the lymphocytes. Further, the point just alluded to, *re* the different disposition of the included corpuscles in the two cases. Lastly, an interesting point is that recently ingested corpuscles in large mononuclears which still retain their hæmoglobin, always stain uniformly black after iron-hæmatoxylin (fig. 9*b*); that is, none of this stain has been extracted—unlike what is the case with the free corpuscles. On the other hand Kurloff-bodies (or corpuscles becoming altered into such), of the size of a single corpuscle are invariably red entirely. It is only the larger bodies, with a correspondingly greater amount of the substance filling the "vacuole," in which more or less of the black may be still left. For some reason this substance in a Kurloff-body does not retain the black stain quite so firmly as does the corresponding quantity of hæmoglobin.

What then are the characteristic "inclusions" which appear pale in films and yet stain so strongly with Giemsa? It was already clear that they did not consist of chromatin, but I applied the methyl-green test to see their behaviour under the action of this stain. I found that they stained yellowish, a quite different colour from the bright green of the nuclei; and they stood out well from the colourless "vacuole" in which they lay. These inclusions are definite homogeneous elements, not of chromatinic nature, which appear in smears and films in just the same various forms in which they occur naturally. In one case after I had treated one half of a freshly made smear in the above manner, I fixed this same half in alcohol and stained it with Giemsa, and in particular Kurloff-bodies whose position I had noted the appearance of the inclusions was precisely the same.

The whole explanation of the Kurloff-bodies is undoubtedly as follows: *they are the result of an unsuccessful attempt on the part of the lymphocytes of the guinea-pig to digest red corpuscles upon which these cells have a predilection for feeding.*[1] The nature of the alteration which takes place is quite different from that which occurs when the hæmoglobin is properly digested by the macrophages; with the exception that neither in this case is any pigment produced. By the action of some ferment the hæmoglobin is broken down to a certain extent. This is, however, the only share the lymphocyte takes in the production of the Kurloff-body; and this is, apparently, as far as it can go in the direction of metabolizing this food. The alteration in composition begins very rapidly, much more so than in the case of the digestion of corpuscles by large mononuclears. Hence the earliest stages are very difficult to find.

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[1] Probably in this case also only, or mainly, effete corpuscles are taken up. The abundant occurrence of the Kurloff-bodies in the spleen suggests that this is so.

As with the black-staining substance in the platelets, so in the case of the homogeneous contents of the "vacuole," which stain uniformly black with iron-hæmatoxylin (or purple with Delafield), and which are also intensely eosinophil, I inferred that this substance contains the iron of the hæmoglobin, and not the pale inclusions. Here, too, this view is shown to be correct on the application of the microchemical test, as above described (p. 334). In films, the contents of the "vacuole" stand out a conspicuous, uniform blue, even more marked than is the chromatin of the nucleus, and completely hiding the colourless inclusions. The colour is deepest in the larger Kurloff-bodies, owing, of course, to their greater size. Judging from the degree of the reaction, I should say that the iron-compound here is one from which the "masked" iron is liberated more readily than in the case of the iron-compound present in platelet-cytoplasm.

The principal difference, therefore, between the digestion of the hæmoglobin by the macrophages, and its alteration into a Kurloff-body by the lymphocytes is as follows: In the former case, the hæmoglobin is metabolized entirely, the iron becoming incorporated into or, at any rate, associated with the cytoplasm in some still complex proteid combination. In the Kurloff-body, the iron is at once split off from a considerable part, at all events, of the protein-substance which helped to constitute the hæmoglobin; though even in the liquid contents of the "vacuole," the iron is still more "masked" than "free." The characteristic, red-staining inclusions (after Giemsa) represent this remaining proteid, which may be itself the product of some interaction between the original proteid and the "digestive" ferment.

This difference in the result of the hæmatophagic mode of behaviour explains, I think, why the formation of the Kurloff-body is so much more rapid a process than the digestion of the red corpuscles by the large mononuclears, which according to Boycott and others, probably takes anything up to a week to complete. In this latter case, the whole of the hæmoglobin, iron included, has to be metabolized in such a way that it can be assimilated. The lymphocyte, on the other hand, is apparently quite unable to assimilate and make use of this food. There the resultant Kurloff-body remains, as a cytoplasmic inclusion, increasing in size as more corpuscles are ingested and added to the mass: it never becomes less, by gradual absorption into the cytoplasm. The lymphocyte, it must be inferred, continues to take in nutriment by osmosis from the plasma. On the other hand, the Kurloff-body does not seem to harm the cell in any way, which can, apparently, divide while containing one (fig. 12*b*). When the mass has become of an inconvenient size, it is simply abstricted; this may indeed happen before the Kurloff-body has become anything like the full size to which it may attain; and it probably always happens if the lymphocyte divides.

The Kurloff-bodies occur free, that is, separated from the lymphocytes in which they have been formed. I have seen such free bodies occasionally in the peripheral blood; and in spleen-preparations, both smears and

films (fig. 11), they occur commonly and of varying size. These free bodies show a definite limit to the "vacuole"; in smears this border is often stained faintly blue. Doubtless, a minute portion of the cytoplasm of the lymphocyte, in the form of a delicate enclosing envelope, is abstricted along with the Kurloff-body. Ultimately these free bodies and their contents most probably break up altogether and are dissipated in the blood.

I have not been able to determine whether there is any definite sequence of change in the form and number of the masses and granules comprising the inclusions; or whether the varied appearances result, in the main, from the manner in which the proteid is separated (or formed) from the hæmoglobin. From a comparison of corresponding smears and films, where the latter show very early stages (corpuscular masses still yellow in tint), I rather think that, in the early stage, there is a finely granular condition of the inclusions. If I am correct, the larger masses and curved rods, etc., are perhaps formed by coalescence in various ways of the original granules.

EXPLANATION OF FIGURES.

FIG. 1.—Platelets, and a lymphocyte for comparison. The platelet-granules are distinct in character from those of the lymphocyte. (Smear, Giemsa; × 1,600.)

FIG. 2.—Platelets, showing the appearance according to the amount of extraction of the black stain (see text). Corpuscles, polymorph, and lymphocyte for comparison. (Film, iron-hæmatox. + eosin; × 1,600.)

FIG. 3.—A field in a dehæmoglobinized smear. (Iron-hæmatox., undifferentiated; × 1,600.)

FIG. 4.—(*a*) Nucleated reds, red corpuscles and fragment of same. The other figures show the ingestion of corpuscles and free nuclei by the large mononuclears, and the formation of platelet-granules in the cytoplasm, as a result of digestion; (*b*) cell containing several unaltered (recently ingested) corpuscles, or portions of corpuscles, each separate from the others; (*c*) cell containing an ingested nucleus and also a fragment of a corpuscle; (*d*) portions of abstricted platelet-cytoplasm, of varying size, down to that of a discrete platelet (*e*). (Smear, Giemsa; × 1,600.)

FIG. 5.—Nucleated reds (normoblasts) (*a*), and free nucleus of the same (*b*). Megakaryocytes to show cell-history, beginning as cell of large mononuclear or "transitional" type; (*c*) large amœboid form, containing two lymphocyte-nuclei; (*d*) platelet-cytoplasm, in different stages, being thrown off; (*e*) a hungry megakaryocyte, containing several recently ingested, immature red-cell nuclei; (*f*) ultimate disintegration of a megakaryocyte. (Film; × 1,600.)

FIG. 6.—Megakaryocyte (medium size) containing an ingested corpuscle. (Film, Giemsa; × 1,000.)

FIG. 7.—Bone-marrow of a purpuric guinea-pig, showing two large megakaryocytes. The one to the right contains a huge "vacuole," with nine included eosinophil polymorphs; and another is present in the cytoplasm below. That to the left contains a mass of ingested corpuscles (indicated by an arrow), the upper part being denser than the lower; the dark body to the right of the mass is the nucleus of the cell. (Dr. Ledingham's section; × 500.)

FIG. 8.—Bone-marrow of purpuric guinea-pig, also showing two megakaryocytes. The upper one contains four eosinophils inside the nuclear ring; the lower one possesses a huge ring-like nuclear complex, and in its centre are two recently ingested red corpuscles (indicated by an arrow). Two eosinophils are seen in the cytoplasm just above. (Dr. Ledingham's section; × 500.)

FIG. 9 (on same plate as fig. 6).—Megakaryocyte, showing platelet-cytoplasm and aggregations of platelet-granules at the periphery; the clearer area, to the left of the nucleus, indicates recently ingested corpuscles. (Smear; × 800.)

FIG. 10.—Kurloff-body (medium size) in lymphocyte. (Smear; × 1,600).

FIG. 11.—Red corpuscles (*a*); large mononuclears containing ingested corpuscles (*b*), one of which shows platelet-cytoplasm fraying out at the right-hand side; (*K*) various conditions of the Kurloff bodies in lymphocytes; (*f.K.*) two free Kurloff bodies. (Film; × 1,600.)

FIG. 12.—A, Large Kurloff body, in a lymphocyte about to divide, showing the pale inclusions in the rose-coloured contents of the "vacuole"; B, Kurloff body showing distinct granulations in addition to the rose-coloured mass. (A, film, Giemsa; B, smear (see text), Giemsa; × 1,600).

(To be continued.)

THE LOAD CARRIED BY THE SOLDIER.

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(*Continued from p.* 263.)

IX.—Byzantium.

After the submergence of the classical empires by the barbarian hosts who surged over Europe we have comparatively little record of organized armies until the rise of the age of chivalry in mediæval times, with the notable exception of the armies of Byzantium.

The Byzantine forces are of interest as belonging to a people who maintained the standard of Christianity against the Slavs, Saracens and other "barbarians" for many centuries, and whose army, extraordinarily well organized in many respects (e.g., its engineering and medical services), was founded on a Roman substratum by the modifying influences of the Foederati of the later Roman Empire, and of the many and increasingly important peoples beyond the confines of the Empire, and the half Romanized inhabitants of the distant provinces. In the third century after Christ, the heavy armed infantry of the empire still formed the core of battle, but by Vegetius' time were losing their distinctive character. By A.D. 450 the cavalry were all in all, and the very name of legion well nigh forgotten. Vegetius ascribes this to the lack of training and exercise of the legionaries, as a result of which the soldier ceased to wear his armour habitually and grew to find it heavy when the time came to assume it; wherefore the men begged leave first to abandon the use of the cuirass, and then of the helmet. In part, however, it was due to the mode of border warfare and the improving arms of the surrounding peoples. (For discussion see Oman's *Art of War*, Vol. II.) Procopius (*De Bell. Gott.*) tells us that by the sixth century the infantry were so little used that many procured themselves horses and fought as light cavalry.

In general we may say that the Byzantine Army relied on cavalry, and used infantry only in hill warfare. The majority of their enemies were mounted, although the Franks preferred infantry. The latter, according to Agathias, wore but rude protection without mail, and employed spears, swords, and axes, *vide* fig. 4. The great majority of the other foes of Byzantium relied on the impetus of heavily mailed horsemen. To meet such foes the Byzantine cavalry, recruited mostly from men of substance, were required to organize themselves as heavy cavalry. Maurice (*Strategicon*) desired every young man to be trained, for this arm, in the use of the bow and of two javelins. The cavalry required a considerable

train of servants and boys, "some bondsmen, others free hired attendants," and even the poorer troopers had one attendant and pack horse for every four or five of them: the large train was considered justifiable in view of the good condition of the troops which resulted.

FIG. 4.—Frankish Infantry.

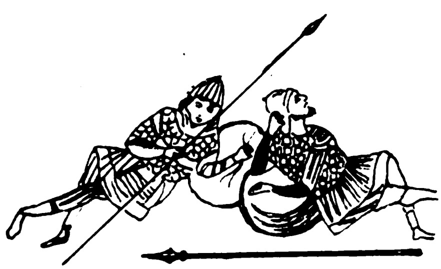

FIG. 5.—Byzantine Infantry.
(After Oman—from Theodore).

The foot soldiers were divided into light troops, mainly archers (*Leo, Tactica*) wearing a knee-length tunic of uniform colour and nailed shoes, and carrying forty arrows, a small round buckler, and an axe; and heavy troops with lance, sword, axe and large shield, together with a mail shirt, leather breeches, and steel helmet (*vide* the illustration from the Psalter of Theodore of Cæsarea, fig. 5). The infantry also had a considerable train

every sixteen men having a ration cart, a cart for trenching tools and cooking gear, etc., a pack horse with eight to ten days' rations, and two attendants. These carts were often used as a laager within which to camp.

X.—The Middle Ages.

The Anglo-Saxon freemen, on the outer edge of Europe, were among the last infantry to succumb before the predominant cavalry of the Normans. As the barrows, and the legends of Beowulf, reveal, these Anglo-Saxons originally used a wooden shield for protection only, "baring their chests" to their enemies, and employed variously spear, sword and knife. (The Bayeux Tapestry reveals their miscellaneous weapons.) But increasing prosperity led to the gradual introduction of chain mail hauberks among them, and the insidious process of loading up appears to have gone on to a considerable extent. As a result King Harold, campaigning against the Welsh, found his troops too encumbered to act effectively and was compelled to reduce their defensive equipment to simple leather flaps on the tunics (*Ingulphus*); fig. 6 illustrates an Anglo-Saxon warrior.

The Battles of Hastings and Dyrrachium finally extinguished the last struggles of infantry, which had been falling into greater disrepute ever since Rome was ravaged by the mounted Northmen; and the organization of the social system of the knightly ages helped to maintain the predominance of cavalry. The feudal system depended, for the "Defence of the Realm," on calling up the landholders and freemen; every man of 15 to 60 years of age who possessed £15 in land and forty marks in goods, had, by the law of Edward I, 1285, to provide himself with an iron hauberk, a sword, and a horse, while the free laymen, according to their state and degree, were obliged to provide arms and armour to different scales. The mounted knights were for long the more important arm, their retainers, the fanti or hired servants (whence "Infantry"?) being regarded merely as adjunct of the equestrian order. The fine stand of the Swiss infantry at Morgarten in 1315, however, indicated the value of disciplined infantry, and led to the gradual restoration of their importance, as also did the fact that at Crecy the horsemen had to dismount and fight as infantry. With the advent of explosive weapons, by which missile force could be employed to act uniformly independent of physical strength, brute force became of gradually less importance, and the heavily (and now uselessly) armoured soldiery of the Middle Ages gave way to the more flexible infantry of modern times. It were well, however, to review briefly the outstanding characteristics of mediæval armies, to learn what they tell.

The knight may be referred to as an interesting figure, but of no particularly real importance to the present inquiry. Each furnished his own weapons, but usually was outfitted on similar lines, although the mode and fashion of body armour changed considerably and forms a study of itself. As a rule a padded frock under the chain mail hauberk took the

shock of blows on the chest; a metal helmet with chain mail curtain or camail, chain pantaloons, greaves and sollerets, and a shield, constituted the remainder of his defence; while a lance for charging with, and a four feet sword together with mace or axe for use after charging, completed an equipment so cumbersome as to limit its tactical use almost to vanishing point. In reality all the knight had to do was to keep his seat and direct his lance; exertions in such stifling conditions can hardly have been

FIG. 6.—Anglo-Saxon Warrior. A.D. 400-1000. (After Luard.)

tolerated and many, indeed, are reported as having been suffocated (? heat-stroke) (*Father Daniel, Mil. France I*). When brought to the ground such knights had to be "cracked open and broken up like lobsters" to be dispatched (*Ph. de Comines*, 1495). The reference to lobsters probably applied equally to the cyanosed colour of the unfortunate knights as to the rigidity of their outer casing. The weight of some of the articles mentioned

may be of interest as indicating something of the equipment of the times. Those of the Earl of Leicester (*c.* 1560) in the Tower collection are given below:—

(1) Body Armour.

	lb.	oz.
Helmet	8	14
Gorget	3	8
Breast and tassets	17	12
Back	9	7
Pauldrons	7	11
,, with arm-pieces	8	5
Gauntlet, one	1	6
Manifer, left hand	3	0
Leg pieces, right	3	10
,, ,, left	3	10
Demi cuisse & Genouilliere, right	1	12
,, ,, left	1	12
Weight of body armour	70	11

(2) Extra Pieces for the Joust.

	lb.	oz.
Grand garde	12	5
Volante piece	4	0
Total	16	5

(3) Arms, etc.

	lb.	oz.
Sword	4	0
Lance	7	0
Vamplate for ditto	1	14
Mace	12	0
Bit and bridle	4	0
Total	28	14

(4) Horse Armour.

	lb.	oz.
Neck piece or crinet	5	6
Chanfrein	6	0
Horse's breast-piece—peytrel	7	12
Saddle	22	0
Total	41	2

The weight carried by the horse appears, therefore, to have been about 360 pounds (man and clothes 150, armour 70, horse-armour 40, arms 30, saddlery and kit 70), and explains the heavy type of animal preferred for the joust (cf., the modern Clydesdale). This is in agreement with the findings of Sir W. Gilbey in "The Shire Horse." But such inquiries are really academic. We know that the knight rarely carried his own equipment, being provided with esquires, custrels and pages for the purpose. Even so, when the armies had to march, large retinues were necessary and yet horses and men were usually distressed (*vide* the description of Edward the Third's first expedition against the Scots). Among those armies of, primarily, mounted men, the lighter army had great advantage, as witness the mobility and success of the Scots Army, 1327, who moved twenty-four miles a day, the whole host being mounted, with no carts or unmounted camp followers, each man carrying his own rations in the form of a saddle-bag of oatmeal, to be cooked on a flat metal grid (*Berner's Chron. I*). Fig. 7 suggests the appearance of a knight (dismounted) of the fifteenth century.

It is in the infantry, however, that our interest mainly lies. They were armed according to their social status, usually with mailcoat, helmet and lance (also a shield if possessing over sixteen marks), all provided by themselves; (the Statutes of Armour were repealed only in the reign of James I). The freemen required the armour to withstand the onslaught of cavalry employing lances and swords. Gradually they tended into two classes, viz., archers and billmen. As to the former, the increasing use of

archers as the characteristic form of light infantry and their relative immunity from cavalry (obtained by improving the temper of the arrow-points so as to penetrate armour), led to the gradual disappearance of heavy armour among them, and to its retention solely among the pikemen. The glory of the English archers ("the flower of feudal infantry," *De Comines*, 1580) was due largely to the fact that archery was a national attribute to which all the freemen were accustomed. The army was still

FIG. 7.—Knight, dismounted, in Gothic armour. *Circa* 1450. (After Scott.)

a national army, and standing armies did not begin to appear until the reign of Edward VI. We have certain vivid pictures of the archers. From all accounts their movements were rapid and effective. Their protective covering was usually limited, *vide* fig. 8. Sometimes they wore a padded tunic and a metal headpiece (the French archer's tunic often had thirty folds [*Mem. of Louis XI*]), and at Agincourt we find the majority "were without armour in their doublets" and "some wore caps of boiled leather, some their feet bare" (*St. Remy, Ed. Buchon, Ch.* 52). Their

weapons were the bow, a sheaf of twenty-four arrows, a sword and dagger, or axe, the latter for use after discharge of the arrows. Obviously the weight carried was very moderate. I estimate it at under thirty pounds. One need disregard any question of personal kit, or anything other than arms and walking clothes; there is no reference to other kit carried on the man; indeed, the first reference to a great coat or cloak is in Elizabethan times, and packs do not appear until the civil war of the seventeenth century. It is notable, too, that a three-horsed cart was laid down for every fifteen

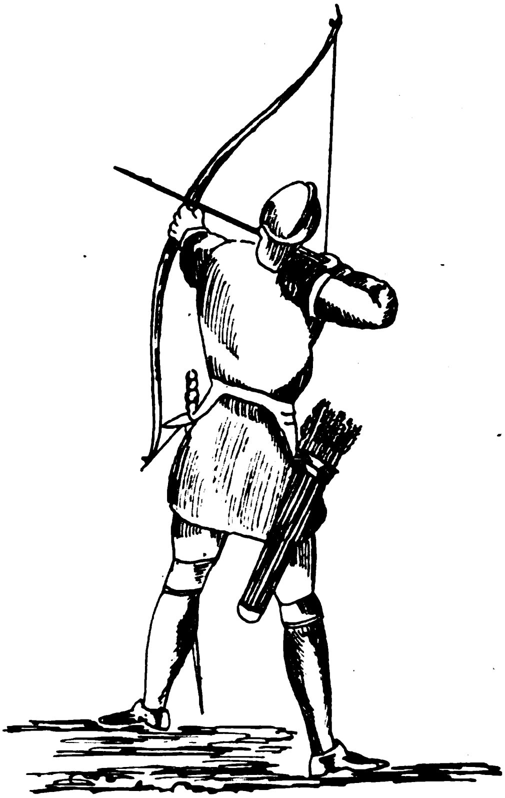

FIG. 8.—Archer. 15th Century. (After Luard.)

archers in an official ordnance of 1475, while the unofficial transport may well be conceived from St. Remy's description of the "12,000 carts laden with arms and clothing," which accompanied 50,000 troops under the Duke of Burgundy in 1411 (*Hist. de Chas.* VI). Sir A. Conan Doyle gives a clear and illuminating description of the auxiliary transport of the "Free Companions" in his admirable "White Company," and when one recalls the custom of foraging on the country for rations, it is evident that the mediæval infantry were not unduly loaded and probably owed much of their vigour in battle to this very fact.

The pikemen and billmen were at first not so important as the archers, and corresponded more to the classical heavy infantry, but played their part in many a strenuous fight. They appear to have been equipped with a metal headpiece, leather doublet with metal breastplate, gorget, and a light shield (*Statute of Winchester*), and armed with a variety of offensive weapons, even slings (*Ballad of Flodden Field*). I estimate their fighting weight as about forty pounds; the march conditions referred to in connexion with the archers apply equally to the billmen, but with the improved organization of the standing armies which become general, facilities for such irregular transport of personal belongings became less simple. As a result there was an evident disinclination to carry the equipment, increased as it was for a phase by the addition of heavier shields (ref. below).

Firearms were introduced about the beginning of the fifteenth century, and modified the constitution of armies considerably. The number of arquebusiers and "shotte-men" gradually rose in proportion and finally they ousted the archers altogether, the last historical reference to archery in war being at the Siege of Devizes in the Civil War (*Gwynne, Mil. Memoirs*) (except of course the use of arrows by the Chinese in 1860, etc.) To meet the early type of firearms large heavy shields (pavois) were adopted. The earlier pavois had usually been of skin-covered wood; the later patterns of metal. A specimen of such a late heavy pavois from Warwick Castle weighs fifty pounds! But, we are told, they were carried by pavisers or servants (*Strutt, Horda II*). With weapons of greater penetrative power they were finally found useless and replaced again by lighter round shields or targes; which in turn disappeared, although still used by the Highlanders in 1745. The troops thus came to comprise mixed bodies of archers, billmen, pikemen, and shot-men; and a review of the levies called up from various countries shows a proportion of two-fifths shot men in Elizabeth's time. The pikes had a long hold, being displaced only slowly by the bayonet, but retained actually as spontons by the officers of the Coldstream Guards until the time of Wellington. A suggestive commentary on the dislike of the infantryman of these days to carry even his very moderate accoutrements on the march is afforded by the fact of the issue of marching-pay, the *raison d'être* of which inducement is given in the "Report of the Arrangements" (*temp. Armada*) as follows: "Whereas the soldiers at all traynings and marches have, verie disorderlie, refused to wear and carry their armour and other warlike furniture . . . whereby the constables have been driven sometymes to carie the same in carts . . . It is therefore ordered that everv soldier shall have over and besides 8d. a day for his wages, a penye a mile for the wearing and carriage of his armour and weapons." Fig. 9 typifies the arquebusier of this period.

Clothing was not as yet standardized, despite the call for uniformity quoted by Rymer (V. 1338), *de una secta vestiri;* but by Elizabethan days there was considerable progress towards uniformity, and we read of

a Lancashire levy, raised for Ireland in 1566, as comprising bowmen "with blue hassocks, sometimes with a stout jerkin of bull-skinne," and pikemen with "blue broadcloth coat, white fustian vest, and blue kersey skirts (*Harleian*, No. 1926). The undergarments, etc., were provided individually; one presumes dependence on local requisition for their renewal. At any rate no pack was carried. In this connexion see also Wolsey's Expedition to France (*temp. Hen. VIII*).

To revert to a general consideration of the armies of the period in question, the striking fact which emerges is the number of camp followers associated with all armies. That such men, with the associated transport, relieved the infantryman on the march of the greater proportion of his equipment (as we now understand it), if not altogether of his heavier

FIG. 9.—Arquebusier or Shotte man. *Circa* 1600. (After Luard.)

armour and arms, we cannot doubt. References to the train are numerous in all contemporary records, and the field of Bannockburn recalls their tactical employment in an effective way. It is on record that the battle turned on the sudden appearance over the "Gillies Hill" of the Scots' camp followers, who were so numerous that they were mistaken by the English for a fresh reinforcing army. Estimates by students of the period reckon the number of camp followers to have been about one half of the Scots' troops, but even so it is a remarkable figure. That a people, poor in material possessions and in the most acute crisis of their national history, when every fit man who could be spared would be presumed on the field of combat, should still have one batman for every two combatants, indi-

cates in some measure the degree to which the soldier leant on assistance for the less urgent and vital matters of his existence. A study of continental armies once again demonstrates the success attending a commander who initiates a phase of mobility among troops more heavily laden (given, of course, the military skill to use such troops). I refer to Gustavus Adolphus of Sweden who made his armies mobile to a degree theretofore unknown among his imperialist enemies, by removing his men's armour, and shortening and lightening their weapons. Thus favoured against his heavily armed and slow moving enemies, Gustavus performed great feats by the celerity of his marches and the vigour of his troops at the end of them. Harte analysed the diary of a Scots unit in his army and showed the daily march to average 16 to 18 miles per day—a distance so superior to the average 3 leagues of contemporary armies as in large measure to explain his great and many successes. The moral to be drawn is the same recurring lesson of military history, which all goes to show the tendency of armies gradually to load up the infantryman till as the General Staff puts it, "the equilibrium between mobility" (which dominates tactics) "and equipment is lost"; and the success awaiting the commander who restores that equilibrium.

(*To be continued.*)

MALARIA IN MACEDONIA, 1915-1919.

PART I.—*continued.*

THE INCIDENCE AND ÆTIOLOGY OF MALARIA IN MACEDONIA.

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(*Continued from p.* 277.)

EFFECT OF COLD AFTER PARTIAL DEVELOPMENT: SECOND EXPERIMENT.

(20) A number of *A. superpictus* collected at Lahanah and *A. maculipennis* at Sacavca, in the Struma valley, were brought down to Salonika and fed on a crescent case on November 27, 1918. The mosquitoes were treated in five batches.

Batch 1.—Incubation at temperature 37° C. November 27: Two *A. maculipennis* and two *A. superpictus* fed on crescent case. Incubated in incubator at 37° C. Died in twelve hours: stomach contents contained oökinetes.

Batch 2.—Incubation at temperature 21·5°—23·7° C. November 27: *A. maculipennis* and *A. superpictus* fed on crescent case; incubated at temperature 21·5° C. to 23·7° C. December 5: Two *A. maculipennis* and two *A. superpictus* dissected; one *A. maculipennis*, numerous small cysts (14—20 μ); salivary glands *nil*. One *A. maculipennis*, many larger pre-sporozoite cysts (28—39 μ); salivary glands *nil*. One *A. superpictus*, many large pre-sporozoite cysts; salivary glands *nil*. One *A. superpictus*, stomach abnormal, no cysts seen; salivary glands *nil*. 6th: Some of the mosquitoes given second feed on same case. 7th: Four *A. maculipennis* and one *A. superpictus* dissected. Two *A. maculipennis*, fair number of cysts (35—45 μ) and one also with flagellate infection; salivary glands *nil*. Two *A. maculipennis nil*, but blood obscured view; salivary glands *nil*. One *A. superpictus nil*, but blood obscured view; salivary glands *nil*. 8th: Two *A. maculipennis* and one *A. superpictus* dissected, all with about ten to twenty cysts; salivary glands *nil*, those in *A. maculipennis* about 30 μ, those in *A. superpictus* about 45 μ. 9th: Two *A. maculipennis* dissected. One with cysts 30—45 μ pre-sporozoite stage; salivary glands *nil*. One with large flagellate infection, no cysts; salivary glands *nil*. 10th: Three *A. maculipennis* dissected. One about ten pre-sporozoite cysts 40 μ. Trematode cyst—ovaries nearly mature, egg cases present; salivary glands *nil*. One with two pre-sporozoite cysts (48 μ and 38·4 μ); salivary glands *nil*. One with large number pre-sporozoite cysts (24—48 μ); salivary glands *nil*. 13th: Five *A. maculipennis* and three *A. superpictus* dissected.

One *A. maculipennis*, large number of cysts 45—50 μ; salivary glands *nil.* One *A. maculipennis*, two cysts only 45—50 μ; salivary glands *nil.* One *A. maculipennis*, two cysts only 32 μ; salivary glands *nil.* One *A. maculipennis* about six cysts in pre-sporozoite stage, some 40—42 μ, others larger, about 70 μ (flattened) with segmenting contents and commencing sporozoite formation; salivary glands *nil.* One *A. maculipennis nil.* One *A. superpictus*, large number of cysts 45—50 μ, some with segmenting contents, others unsegmenting, some with sporozoites; salivary glands *nil.* One *A. superpictus*, one large cyst with commencing sporozoite formation; salivary glands *nil.* One *A. superpictus*, two cysts in stomach, commencing sporozoite formation; salivary glands with sporozoites.

December 14: Four *A. maculipennis* and five *A. superpictus* dissected. One *A. superpictus*, large number mature and nearly mature cysts in stomach; salivary glands with sporozoites. One *A. superpictus*, about ten cysts as above; salivary glands with sporozoites. One *A. superpictus, nil.* One *A. superpictus*, large number of mature and nearly mature cysts; salivary glands with sporozoites. One *A. superpictus*, several cysts as above; salivary glands with sporozoites. One *A. maculipennis*, about twelve cysts segmenting and sporozoite-forming; salivary glands *nil.* One *A. maculipennis*, large number of cysts segmenting and sporozoite-forming; salivary glands *nil.* One *A. maculipennis, nil.* One *A. maculipennis*, single large pre-sporozoite cyst; salivary glands, *nil.* 18th: One *A. maculipennis* dissected, *nil.*

In this batch of twenty-two *A. maculipennis* properly examined, seventeen were infected; while of ten *A. superpictus* nine were infected.

Batch 3.—Room temperature: Average maximum, 18·2° C.; average minimum, 9·6° C. November 27: Four *A. maculipennis* and two *A. superpictus* fed on same case and kept at room temperature. December 5: One *A. maculipennis* dissected—still full of unaltered blood. 10th: One *A. maculipennis* dissected—still full of blood with apparently unchanged red blood corpuscles; many small yeasts. 22nd: Two *A. maculipennis* and one *A. superpictus* dissected—*nil.*

The room temperature was sufficiently low to prevent any commencement of development and, as will be seen in the next batch, it completely arrested a development already begun. The length of time—thirteen days—that blood will remain apparently unaltered in the stomach is remarkable.

Batch 4.—Incubation at temperature 21°-24° C., followed by room temperature 9·7°—24° C., followed by incubation at 21°—24° C. November 27: Three *A. maculipennis* fed on same case, incubated in incubator 21°—24° C. December 7: Placed at room temperature 9·7°—18·7° C. 16th: One *A. maculipennis* dissected, about ten pre-sporozoite cysts; salivary glands *nil.* No growth of cysts since December 7 (size 33—45 μ, see Batch 2, December 7). 22nd: One *A. maculipennis* numerous cysts 19—28·8 μ. Flagellates in stomach, intestine and Malpighian tubes; salivary glands

nil. One *A. maculipennis* returned to incubator, temperature 21°—24° C. 28th: One *A. maculipennis* dissected, numerous cysts (twelve to fifteen) in stomach (size 43—48 μ), also three mature cysts full of sporozoites; salivary glands *nil.*

In this batch there were only three *A. maculipennis*, all of which became infected. The first incubation brought about commencement of development. After exposure to room temperature from December 7—22 there was no change in the cysts. A further incubation of six days enabled the cysts in the single remaining mosquito to develop to maturity.

Batch 5.—Incubation at temperature of 21°—24° C., followed by ice-chest at 5·5° C., followed by incubation again. November 27: Four *A. maculipennis* fed on same case, incubation temperature 21°—24° C. December 7: Placed in ice-chest at temperature 5·5 C. for twelve hours and then back in incubator, temperature 21·5°—22·6° C. 22nd: Three *A. maculipennis* dissected, *nil.* One *A. maculipennis* with single large mature cyst full of sporozoites; salivary glands *nil.* (Two of the mosquitoes showed live trematode cysts adhering to stomach.)

The twelve hours exposure in ice-chest at the low temperature of 5·5° C. did not destroy the cysts in the single mosquito infected.

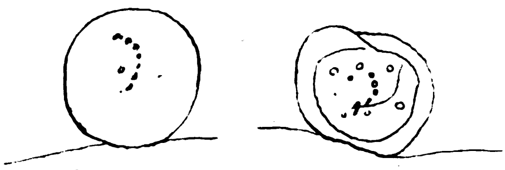

FIG. 19.—Normal (*a*) and degenerate (*b*) cyst of *P. falciparum* to illustrate the change produced after exposure to cold in the ice-chest.

CONCLUSIONS.

Arranged with reference to the question as to whether the cases employed were taking quinine or not, the experiments show that in the case of *P. vivax* of 1 *A. maculipennis* and 12 *A. superpictus* fed throughout on cases taking quinine there were no infections, while of 5 *A. maculipennis* and 15 *A. superpictus* fed at least once on a case not taking quinine (they also fed on cases taking quinine), 3 *A. maculipennis* and 5 *A. superpictus* were infected.

The deduction is that cases of *P. vivax* infection taking quinine, even though gametocytes are present in the blood, are unlikely to infect mosquitoes.[1] In the case of *P. falciparum* the result was very different, for of 40 *A. maculipennis* and 37 *A. superpictus* fed on cases taking quinine

[1] A paper by Bruce Mayne (*Pub. Health Reports*, 1920, July 9, Vol. xxxv., No. 28, pp. 1664-1669), which has recently come to hand, describes experiments proving this very point. A similar observation had previously been made by Schoo.

30 of the former and 26 of the latter became infected. It seems that quinine has a much more marked effect on the gametocytes of *P. vivax* than on those of *P. falciparum*. In the case of the *P. vivax* experiments each mosquito was fed on many more cases than in the *P. falciparum* experiments, so that the chances of their becoming infected were greater.

Experiment 19 demonstrates that a partially developed cyst of *P. falciparum* cannot withstand an exposure of eight days to a temperature of 9° C. to 12° C., for of 4 *A. maculipennis* and 15 *A. superpictus* the cysts degenerated in all, with the possible exception of 1 *A. superpictus* examined after eight days' exposure. In Experiment 20 (Batch 5) it was shown that an exposure to 5·5° C. for twelve hours did not result in degeneration.

King,[1] experimenting in America with *A. quadrimaculatus*, has shown that *P. vivax* will withstand an exposure to a temperature of under 0° C. for four days and to a mean temperature of between 7° C. and 8° C. for seventeen days and that the oöcysts will continue their development at favourable temperatures. *P. falciparum* withstood a temperature of 1·7° C. for twenty-four hours.

Mitzmain[2] tested the effect of long exposure of *P. falciparum* in the mosquito to intermitttent low temperatures but did not obtain any subsequent development at favourable temperatures.

Experiment 20 (Batch 4) shows that a development of *P. falciparum* already commenced in mosquitoes could be completely arrested for fifteen days by exposure to the ordinary winter room temperature (9·7° C.—18·7° C.). The room temperature was sufficiently low to prevent any development commencing in mosquitoes (Experiment 20, Batch 3).

It is evident therefore that in nature a development may commence in the mosquito, be completely arrested, and then continue when the temperature conditions become more favourable. It has been stated above that cysts were discovered all through the winter in the stomachs of mosquitoes hibernating in Lahanah village. The temperature in the barns from which the mosquitoes were collected was never very low, especially if a buffalo or other large animal was there, and this was often the case. The cysts in the mosquitoes were always normal in appearance and showed nothing of the degenerative appearance which came to light in the experiments just detailed. So there is no reason to doubt that the cysts in the hibernating mosquitoes would continue their development when the weather became warmer and that in this way, quite apart from a human carrier, malaria can be maintained through the winter in the mosquito alone. These results have been referred to in a letter to the *Lancet*, July 3, 1920.

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[1] "The Effect of Cold upon Malaria Parasites in the Mosquito Host." *Journ. Exper. Med.*, 1917, vol. xxv., p. 495.

[2] "The Malaria Parasite in the Mosquito: the Effect of Low Temperature and other Factors on its Development," *Public Health Report*, August 31, 1917, vol. xxxii, No. 35, pp. 1400—1413.

Mitzmain[1] records the examination of 1,211 hibernating anophelines in the Mississippi delta prior to May 15 with the object of discovering any evidence of winter infection. He found no infection and concludes that in the area investigated man is the sole winter carrier of malaria. In his paper on paludism in Macedonia already referred to above (p. 184) Cardamatis records the presence of degenerate oöcysts in hibernating anophelines. In my observations no case of degenerate oöcysts was encountered in naturally infected hibernating mosquitoes. It is very probable, however, that it is only in the warmer barns that oöcysts would survive the winter.

A further point of interest is that the development of *P. falciparum* in *A. superpictus* proceeds more quickly than in *A. maculipennis*. In Experiment 20 (Batch 2) it will be seen that on December 14, 1918, *A. superpictus* already had sporozoites in the salivary glands while *A. maculipennis* on the same date had only cysts in which sporozoite formation was taking place. The salivary glands were still uninfected. From this one might conclude that *A. superpictus* was a more ready carrier of *P. falciparum* than *A. maculipennis*.

As regards the relative infectivity of these mosquitoes with the two malaria parasites, if we ignore the experiments with *P. vivax* in quinine cases the total of experiments gives the following figures:—

	A. maculipennis		*A. superpictus*	
	Fed	Infected	Fed	Infected
P. falciparum	40	30	37	26
P. vivax	5	3	15	5

From these figures it seems that both mosquitoes infect more readily with *P. falciparum*, but this may be merely dependent on the extent of the gametocyte infection in the cases used for feeding.

Jancso[2] in a paper on the effect of temperature on the development of malaria parasites in mosquitoes, and Roubaud in his paper referred to on p. 264, state that *P. vivax* develops more quickly in *A. maculipennis* than *P. falciparum* and the latter attributes the early incidence of *P. vivax* infection to this difference. He found that at a mean temperature of 17° C.—20° C. *P. vivax* developed completely in a *A. maculipennis* in fifteen days while *P. falciparum* takes twenty days. In my experiments (Experiment 7) at a temperature of 18° C.—25° C. *P. vivax* completed its development in *A. maculipennis* in seventeen days while *P. falciparum* (Experiment 20, Batch 2), at a temperature which was approximately the same (21·5° C.—23·7° C.) had in eighteen days only reached a stage in which the cysts were

[1] "Is Mosquito or Man the Winter Carrier of Malaria Organisms?" *U.S. Public Health Serv., Public Health Bull.*, No. 84, 1916, December.

"Anopheles as a Winter Carrier of Plasmodium—The mosquito as a Prophylactic Indicator." *U.S. Public Health Serv., Public Health Report*, vol. xxx, No. 29, July 16, 1915.

[2] *Cent. f. Bakt. Orig.*, vol. xxxviii., 1905, p. 650.

commencing to form sporozoites. With *A. superpictus*, however, the difference was not well marked. In Experiment 4 *P. vivax* completed the development in *A. superpictus* in fifteeen days at a temperature of 21° C.—29·7° C., while in Experiment 20 (Batch 2), *P. falciparum* had completed development in the same mosquito at a lower temperature, 21·5° C. to 23·7° C. in seventeen days.[1] It will be seen that the difference in the rate of development of the two parasites in *A. maculipennis* agrees with Roubaud's finding but that in *A. superpictus* there is practically no difference.

Jancso found that 15° C. to 16° C. was the lowest temperature at which development of malarial parasites would take place in *A. maculi-*

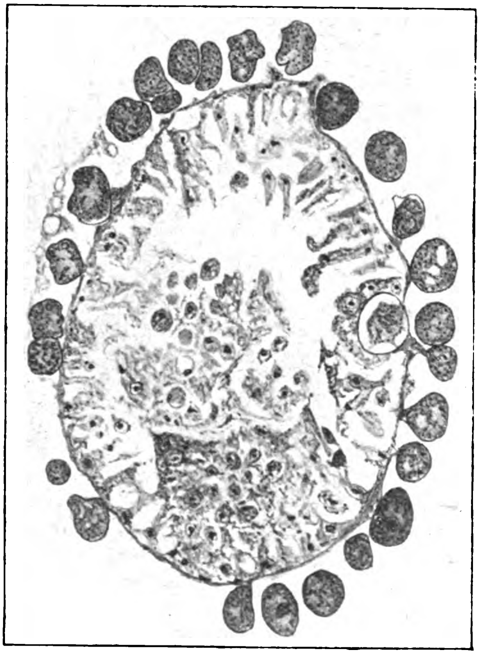


FIG. 20.—Transverse section of stomach of *A. superpictus* showing very heavy infection with oöcysts of *P. falciparum*. One cyst is seen to be developing within the epithelial cells of the stomach.

pennis, and he estimated that fifty-three days would be required for complete development of *P. vivax* at this temperature. In my experience (Experiment 20, Batch 3) a temperature ranging between 9·6° C. and 18·2° C. was sufficiently low to prevent any development taking place.

[1] James ("Malaria at Home and Abroad," p. 14) says: "Wenyon found in the Balkans that forty days were required for the parasite to attain maturity at a temperature of 62° F." None of my experiments bears this out and I think he must have been mistaken in his source of the information. (See letter to *Lancet*).

When several mosquitoes had become infected after feeding on the same case at the same time, it was observed that the degree of infection in the mosquitoes varied. Some of them would have an enormous infection of cysts while others would show only a few. In some cases no infection had taken place. What are the factors regulating the degree of infection of a mosquito I have not the slightest idea. In one case a mosquito which was expected to be infected, but was not, had the whole stomach lined with attached flagellates (*Crithidia fasciculata*). It was thought that this might have prevented infection but in another case malarial oöcysts were present with a similar flagellate infection. The flagellate was seen both in *A. maculipennis* and *A. superpictus*.

The Development of the Malarial Parasite in Macedonian Anopheles.

In the preceding section I have discussed some of the conditions affecting the development of *P. vivax* and *P. falciparum* in *A. maculipennis* and *A. superpictus*. I wish now to make some remarks about the actual development of the malarial parasites in the mosquitoes. The most exhaustive account of this is to be found in the very excellent monograph by Grassi [1] of his observations in Italy. I have very little to add to his account, which covers every stage of development in the mosquito, but in one or two points I may be able to amplify his description. In association with Bignami and Bastianelli he was the first observer to demonstrate that the human malarial parasite completed its development in anopheline mosquitoes, as Ross had done for bird malaria in culex.

During the dissection of the mosquitoes in the experiments detailed above I was able to obtain preparations, some of them very good, of the various stages of development.

For the dissection I used a binocular dissecting microscope, which is almost indispensable for such work. It is possible to remove the stomach and salivary glands of mosquitoes without the aid of such an instrument but the accuracy of the work is increased a hundred fold by the use of a good dissecting microscope and the actual dissection is very much simpler. It is quite easy in less than two minutes to obtain a preparation mounted in saline of the stomach and both salivary glands. By using an instrument of this kind one sees clearly what one is doing and the number of failures in dissection is practically nil.

The salivary glands were removed by drawing off the head of the mosquito in a drop of saline solution with a needle in the usual manner, and if the glands came away with the head they were cut off with the needle before it was raised from the slide. Usually, however, the glands

[1] "Studi di uno Zoologo sulla Malaria," *Reale Accademia dei Lincei* (Anno cccxcvi, 1900).

remained behind in the thorax and it was then a simple matter to extract them from the lower anterior part of the thorax. The mosquito was then turned round and the stomach drawn out from its posterior end. The remains of the mosquito were cleared away and a cover-glass laid on. The preparation was then examined with the various powers of the microscope. A $\frac{2}{3}$ inch was generally sufficient to show cysts in the stomach and a $\frac{1}{6}$ inch sporozoites in the salivary glands. In cases of doubt a $\frac{1}{12}$ inch could be used. If the organs were not sufficiently flattened some of the saline could be withdrawn with a piece of blotting paper.

A preparation like this could be permanently fixed in the following manner. A tiny drop of Schaudinn's fluid was placed at one edge of the cover slip and an equal amount of saline drawn away from the other with blotting paper. The process was repeated several times, the preparation being still under the dissecting microscope. When the fixing fluid reaches the organs they will be seen to change colour and become more opaque. The important point in the process is not at any time to add much fixing fluid, as the cover-glass will rise up and the stomach contract. It is necessary to keep it flattened till fixation has taken place, otherwise the preparation will be too thick for satisfactory examination afterwards. When fixation has taken place, as shown by the change in the tissue, more fixing fluid is added to float up the cover-glass on its surface and it will generally be found that the organs are fixed to it and not to the slide. It is allowed to remain floating for ten minutes or a quarter of an hour till fixation is complete and then very gently taken up with forceps and washed in several successive watch-glasses full of seventy per cent alcohol till all sublimate is removed. This is hastened by adding to one of the watch-glasses a few drops of iodine solution, which itself must be removed in the succeeding washings. The cover-glass preparation can then be brought down gradually to water and stained in various ways, hæmalum, carmine, safranin, etc. After staining it is taken up through alcohols of increasing strength, dehydrated in absolute alcohol, cleared in xylol and mounted in balsam.

For sections the stomach need not be kept flattened under a cover-glass. It is placed directly in the fixing fluid. It is then treated as a piece of tissue, embedded in paraffin and serial sections cut. These may be stained by any ordinary stain but the most satisfactory has been Mayer's acid hæmalum allowed to act for three or four hours.

Preparations of the entire salivary glands will probably be obtained along with the stomach on the cover-glass preparation, but the most satisfactory results are secured from sections. It is almost impossible to embed the isolated glands, so the following method was adopted. The head of the mosquito is cut off without attempting to withdraw the glands. The whole muscular mass and contents of the thorax are then dissected out, care being taken not to disturb the glands, which are included with the mass removed. The whole mass is then fixed and this is embedded

and cut in the usual manner. In this way very beautiful sections of infected glands can be obtained. Mayer's acid hæmalum gives very satisfactory results.

For the study of the oökinetes in the stomach smears of the contents, which, of course, are mostly altered blood, are made on cover-glasses and without allowing these to dry they are dropped film side down on to the fixing fluid. They are then treated as I have described above.

Wet fixed preparations of the sporozoites in the salivary glands can be made in the same way. Having isolated an infected gland in a preparation the cover-glass is carefully removed by sliding and the gland left on the slide in a drop of saline. As much of the saline as possible is sucked up, leaving the glands in a tiny drop. The slide is cleared of all detritus around it and then the gland is broken up with a needle so as to liberate the sporozoites. The tiny drop is spread out quickly and the slide placed film side down on to the fixing fluid. These operations have to be carried out quickly to prevent drying, which will spoil the subsequent result.

Very beautiful preparations of sporozoites, though inaccurate from a histological point of view, can be obtained by allowing the film to dry after spreading and staining with Romanowsky stain (see Plate V). Similarly, preparations of the oökinetes in the stomach contents can be obtained in the same way by staining dried films as with ordinary blood smears, but it must always be remembered, and this holds good for blood films made from the human host as well, that though dried smears stained by Romanowsky stain are often very pretty as regards colouring and are excellent for diagnostic purposes they are misleading from the point of view of actual structure of nuclei and cytoplasm (see Plate I).

Having made these preliminary remarks about the methods used in studying the development of malaria in the mosquito I will now describe some of my results.

It is well known that the first stage of development in the mosquito, and I am speaking now of *P. falciparum*, is the rounding off of the female crescent and the production of male gametes by the male. These processes can be studied on a simple cover-glass preparation of fresh blood, which can be smeared and stained at various stages. In one preparation made in this way by the dry method cases of the actual fertilization of the female by the male were found. Two of these have been depicted in Plate I. They show the male gamete as a long thin thread with chromatin material at its centre. Some published figures of the male gametes, as for instance those of Schaudinn, show a more irregular distribution of the chromatin along the body of the gamete. In my preparations the chromatin appears to be concentrated in a more limited area. After fertilization the zygote formed becomes the motile oökinete or travelling vermicule, which is destined to pass through the epithelial lining of the stomach and settle down between the epithelial cells and the elastic membrane covering the outer surface of the stomach. Preparations of dried films showing oökinetes were obtained from mosquitoes twelve hours after feeding

PLATE I.

FIG. 1.

FIG. 3.

FIG. 4.

FIG. 5.

FIG. 6.

FIG. 7.

FIG. 2.

FIG. 8.

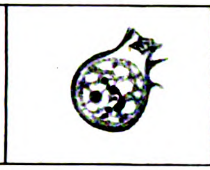


FIG. 9.

FIG. 10.

FIG. 11.

FIG. 12.

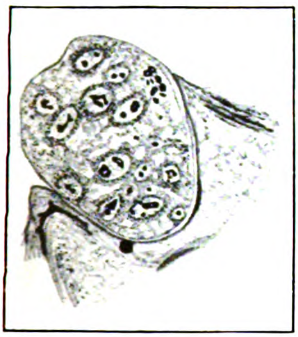


FIG. 14.

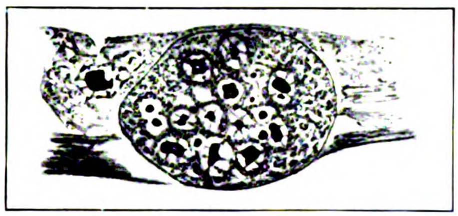


FIG. 13.

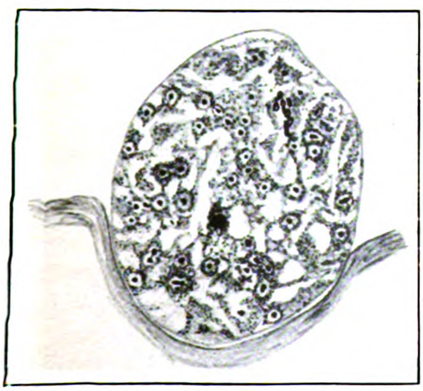


FIG. 15.

10 μ

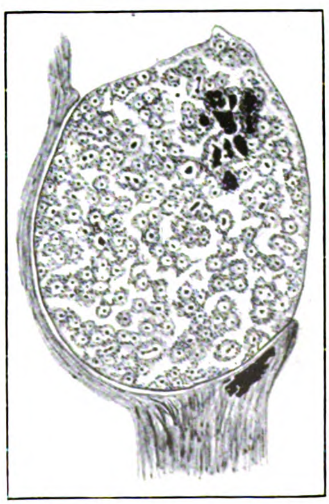


FIG. 16.

To illustrate
"The Incidence and Ætiology of Malaria in Macedonia," by Temp. Colonel C. M. WENYON.

PLATE II.

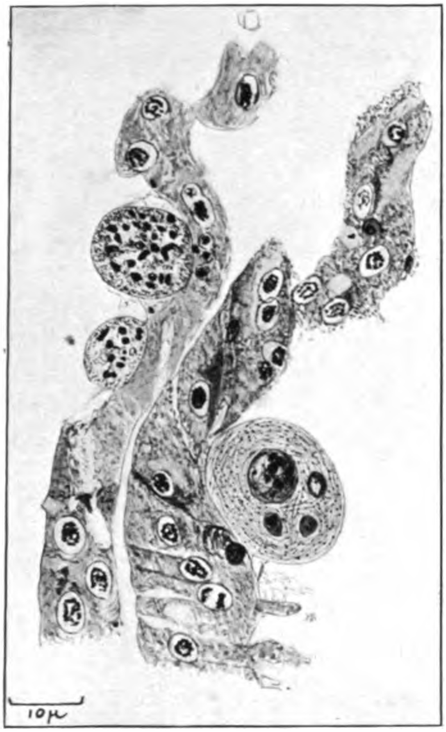


Fig. 1.

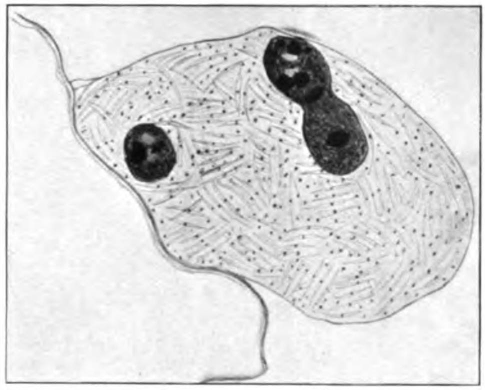


Fig. 2.

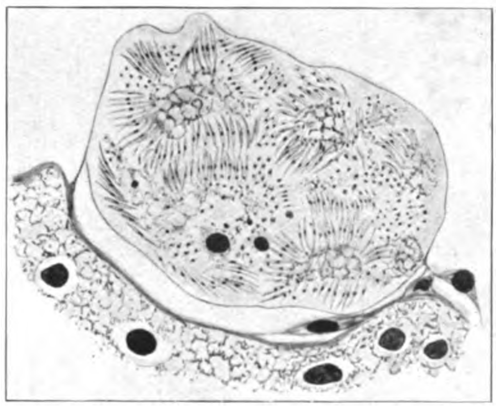


Fig. 3.

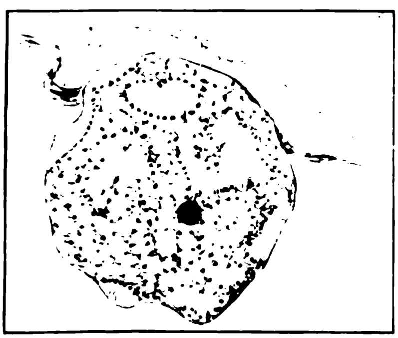


Fig. 4.

Fig. 5.

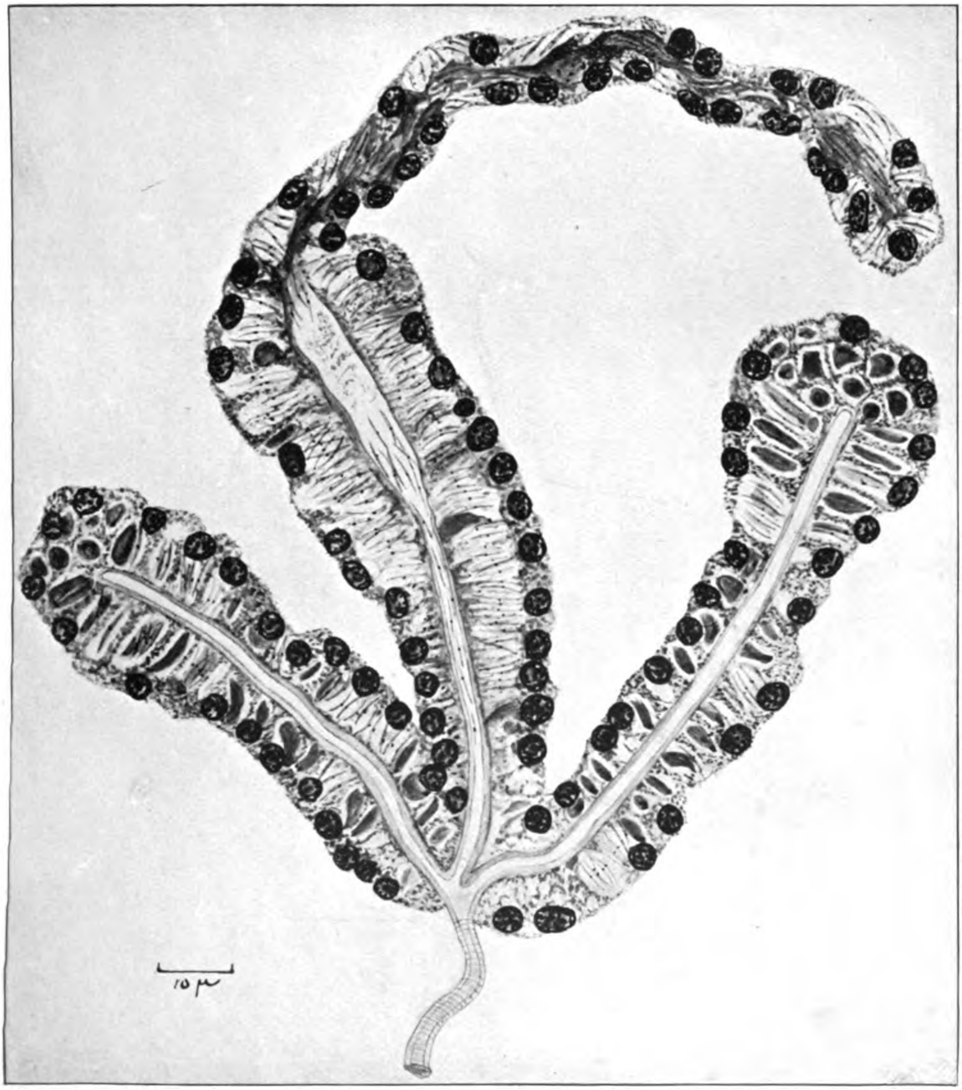
10 μ

The scale on fig. 1 refers also to figs. 2-4.

To illustrate

"The Incidence and Ætiology of Malaria in Macedonia," by Temp. Colonel C. M. Wenyon.

PLATE III.



To illustrate
"The Incidence and Ætiology of Malaria in Macedonia," by Temp. Colonel C. M. WENYON.

PLATE IV.

FIG. 1.

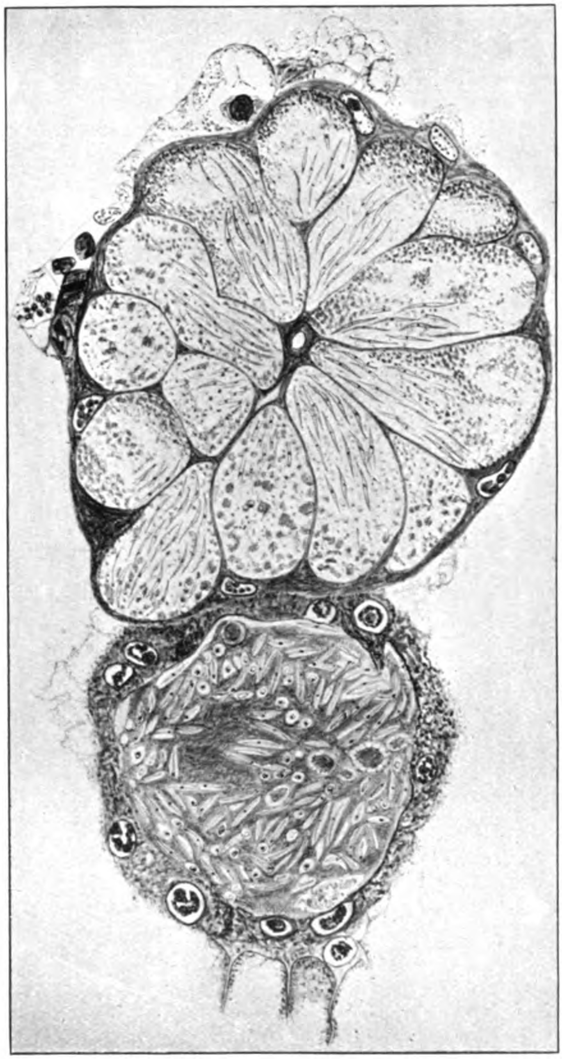


FIG. 3.

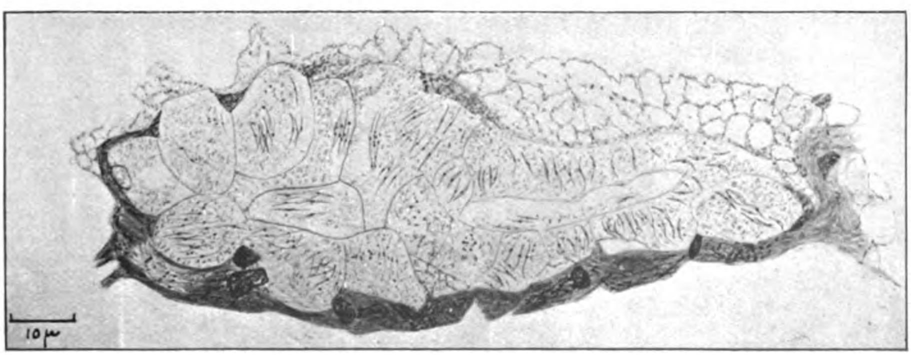
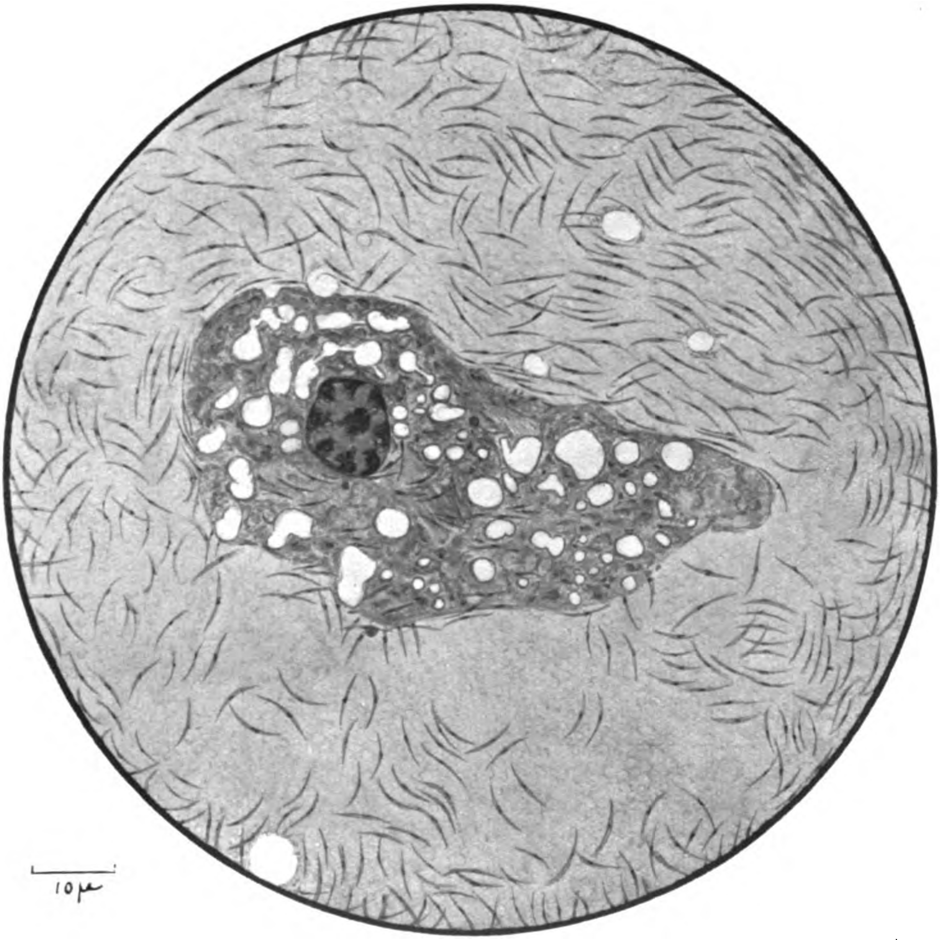


FIG. 2.

The scale on fig. 2 refers also to figs. 1 and 3.

To illustrate
"The Incidence and Ætiology of Malaria in Macedonia," by Temp. Colonel C. M. WENYON.

PLATE V.



To illustrate
"The Incidence and Ætiology of Malaria in Macedonia," by Temp. Colonel C. M. WENYON.

(Plate I). It is unfortunate that I omitted to prepare wet films of the stomach contents, so that I can say nothing about the actual nuclear structure of this stage of development, though the character of the nucleus in the next stage would lead one to suspect that the oökinete had a similar nucleus.

The subsequent stages of development were obtained in sections of the stomach. The earliest is that following on the passage of the oökinete through the lining epithelium of the stomach. Fig. 9 (Plate I) shows such a rounded off oökinete and it will be seen that the single nucleus consists of a central karyosome surrounded by a clear area which is possibly limited by a membrane, though if this is present it is so delicate that it is difficult to distinguish it from the cytoplasm of the parasite. Some fine strands stretch between the karyosome and the outer surface of the nucleus. As development proceeds the nucleus divides by the karyosome elongating and becoming constricted, and finally two nuclei of the same type, but smaller, are produced (fig. 10, Plate I).

In studying Grassi's figures it would appear that these early stages of nuclear division are not clearly shown. It is possible that the fixative had not acted well or had produced some shrinkage which had obscured the actual state of affairs. The two nuclei formed then divide again to form four and repeated nuclear division takes place till an enormous number of minute nuclei arise, each one having essentially the same structure as the first single one. Fig. 11 (Plate I), shows one of the later nuclear divisions on a surface section of cyst. It is evident that the amount of chromatin in the original nucleus would be quite insufficient to supply the chromatin of the large number of nuclei ultimately formed. As nuclear division proceeds additional chromatin is added to the karyosome, and somewhat irregularly, for at what may be called the middle period of nuclear division the karyosomes of the various nuclei are rather irregular in form and size (figs. 12, 13, 14, Plate I). In spite of this irregularity nuclear division proceeds in the manner indicated till finally in the later stages the nuclei begin to have a more regular appearance. The last nuclei to be formed are those destined to become the nuclei of the sporozoites and each one has a tiny central karyosome surrounded by a clear area which in its turn is limited by a very delicate membrane (fig. 16, Plate I).

While the nuclei are dividing changes are taking place in the cytoplasm. These consist at first in the appearance of vacuoles, which increase in number, break into one another, and also externally, and finally reduce the cytoplasm to the condition of a sponge. There are then numerous branches and septa running in all directions while at their points of junction there are collections of cytoplasm which give one the impression of separate bodies united by strands. In thin sections of a structure like this the collections of cytoplasm at the nodes frequently appear as separate bodies unconnected with others, and this appearance has given rise to an error. It is frequently stated, and Grassi seems to support this view, that

actual sporoblasts are produced in the cysts. Now sporoblasts, as the term is used in descriptions of the development of sporozoa, are definite bodies into which the sporonts divide, and each sporoblast frequently forms for itself a secondary cyst called a sporocyst. The sporoblasts resemble one another very closely and each one ultimately gives rise to a definite number of sporozoites. Furthermore, when the sporont divides into sporoblasts it does so by multiple segmentation into the particular number of sporoblasts and not by successive divisions. Now the bodies produced in the malarial parasites do not correspond in any way with sporoblasts, though it must be admitted that in sections the thickened nodes have sometimes a resemblance to sporoblasts. As a matter of fact, there is no production of sporoblasts in the malarial parasite for, as we have seen, the whole body of the parasite is a sponge-work with aggregations of cytoplasm at the nodes. Nuclear division is proceeding while this vacuolation is developing and the majority of the nuclei are dividing in the thickened nodes in which they are distributed. Figs. 12 to 16, Plate I, are drawings of sections of cysts in the later stages of development and they show the network and thickened nodes with multiplying nuclei. Some of the vacuoles are larger than others and they stand out clearly. Sometimes there is developed a single large central vacuole but the cytoplasm surrounding it is honeycombed with smaller vacuoles of varying size. There is no uniformity in the production of these vacuoles.

Finally, the last nuclear division takes place and the tiny sporozoite nuclei are produced. These arrange themselves on the surface of the cytoplasm limiting the vacuoles. Grassi again speaks of them as sporoblasts and states that each one by elongation becomes transformed into a sporozoite. This is probably not correct. Each is a nucleus and not a sporoblast and the sporozoites are formed, as frequently occurs in other sporozoa, by little finger-like processes of cytoplasm growing up from the surface, gradually increasing in length and so absorbing the cytoplasm from which they arise. When they are about half their ultimate length the nucleus passes into them and they grow still further till they reach the length of the mature sporozoite. By this time so much of the cytoplasm is absorbed in the bodies of the sporozoites that little remains of the original meshwork which, breaking down, leaves small residual masses to which clusters of sporozoites are attached. In the mature cyst the sporozoites have freed themselves from the residual masses and they fill the cyst in an irregular tangle, while several residual bodies occur here and there (figs. 3 and 4, Plate II).

The formation of sporozoites in the manner described furnishes an explanation of the vacuoles. For this to take place a surface of cytoplasm is required and it is to increase the available surface, to accommodate the large number of nuclei, that the vacuoles are formed. It should be mentioned that not only do the sporozoites grow out into the vacuoles but that they also grow from the outer surface of the sporont between it and the cyst wall.

During the nuclear division and increase in the amount of chromatin in the karyosomes it appears that certain nuclei cease to divide and, still increasing in size, become comparatively large bodies which are ultimately seen in the residual masses of cytoplasm in the mature cyst. These are seen in fig. 15, Plate I, and figs. 2-4, Plate II, but what their function is it is difficult to imagine. When the cyst ruptures into the body cavity of the mosquito and the sporozoites are liberated to seek the salivary glands they are lost.

During the whole of this development the pigment of the original female gametocyte is present in the cyst. As has been noted by other observers, it is often arranged in a line or string of granules. It is finally lodged in one or more of the residual bodies in the mature cyst, and at this stage may be difficult to detect.

The above is a description of the development of the oöcyst of the malarial parasite as I have followed it. It agrees, except in details, with Grassi's original description. The development was followed chiefly with *P. falciparum* but *P. vivax* develops in exactly the same manner though, as noted above, a little more quickly than *P. falciparum*. The same process of development occurs in the case of *P. præcox* of birds in culex, except that here again, in my experience, the development is still more rapid than in the case of the human parasites.

What might be called the normal situation of development of the oöcyst is, as is well known, between the epithelium of the posterior end of the stomach and the elastic membrane. In some cases, especially when there is a large infection, development will actually take place in the epithelial cells and in such a manner that rupture of the cyst would lead to escape of sporozoites into the stomach and not into the body cavity. Fig. 20 shows cysts developing in such a situation.

After leaving the ruptured cysts the sporozoites enter the salivary glands where they lie, as Grassi has described, in the gland cells (Plates III and IV). The salivary glands themselves vary very much in appearance. Sometimes the cells appear only finely granular, at other times they are more coarsely granular, while again they may show large vacuoles filled with a secretion which is coagulated in fixation. This secretion may collect in the duct in large amount, so that it is considerably dilated, and the gland cells are reduced to a mere layer around this dilated duct (fig. 3, Plate IV).

Sporozoites are distributed all through the cell cytoplasm and the secretion in the cells and within the duct. The sporozoites are sometimes only few in number, at other times they are very numerous, as Plate V, which is a drawing of a dried smear, shows.

Wet fixed films of sporozoites were obtained from salivary glands. Prepared in this way the sporozoite has a central nucleus which has a structure like that of the small nuclei in the final stage of development of the cyst. There is a central karyosome, a clear area and a fine membrane. The nucleus is slightly elongated owing to the narrowness of the sporozoite,

but its structure clearly proves that the small spherical bodies seen in the cyst after the last nuclear division are in reality nuclei and not sporoblasts which become themselves transformed into sporozoites (fig. 5, Plate II).

On only one occasion was a cyst containing brown spores met with (Experiment 15). This was a case of experimental infection with *P. falciparum*. Normal cysts were present at the same time. The bodies within the cyst were brown in colour, the majority were elongate sausage-shaped structures, but others were more rounded. They each had a clear central core limited by a brown granular membrane. The membrane, however, had no very definite outline and was quite unlike the membrane limiting the spore of any sporozoon. It is highly improbable that the bodies are parasitic and Grassi's view is probably the correct one, that they represent some degenerative change which has taken place in the cyst. The elongate nature of some of them might suggest a deposit of some kind enclosing sporozoites, but this is again unlikely since the normal cysts had not yet reached the sporozoite stage. Grassi's figures show the appearance of these bodies very well.

During the dissection of mosquitoes for the determination of the incidence of infection in nature a very misleading appearance was met with on several occasions. Salivary glands from both *A. maculipennis* and *A. superpictus* were seen which, on first inspection, appeared to contain sporozoites. On closer examination it was seen that the structures resembling sporozoites were much more rigid, were uniformly straight and varied in length. They turned out to be acicular crystals of some kind but they occupied the cells just as the sporozoites do and in a hasty examination they could not fail to be mistaken for actual sporozoites. When first seen an attempt was made to stain them but they had disappeared from the preparation. The next time they were encountered they were left in the moist preparation mounted in saline solution and examined from time to time. They were seen to vanish gradually. Further tests showed them to be more quickly soluble in distilled water, while they dissolved instantaneously in dilute acids. Their true nature was thus apparent. I mention them here because they can very readily give rise to fallacies in observation. Similar structures were seen by Christophers and Stephens (1900) in *Culex pipiens* in West Africa. (*Report of Malaria Com. Roy. Soc.*)

In one case a single lobe of the salivary gland of an *A. maculipennis* contained square-ended rods which were undoubtedly long bacilli. These also might possibly be mistaken for sporozoites.

Quite frequently in *A. maculipennis* and less commonly in *A. superpictus* cysts larger than those of the mature malaria parasites were found adhering to the stomach. These contained minute trematodes which could be seen squirming about inside. It is interesting to note that in Experiment 20, (Batch 5), described above, two of the *A. maculipennis* exposed to the ice-chest temperature (5·5° C.) for twelve hours were afterwards found to contain these cysts harbouring live trematodes which were apparently unaffected by the cold.

DESCRIPTION OF PLATES.

Plate I.—All figures drawn to scale shown at 15.

1—8. Dried films stained by Leishman's stain.
9—12. Sections prepared from tissue fixed in Schaudinn's fluid and stained with Mayer's acid hæmalum.
13—16. Sections prepared as above but stained with iron hæmatoxylin and eosin.
1 and 2. Two conditions seen in a dried smear of a wet preparation of *P. falciparum* in which flagellation was seen to have taken place. They undoubtedly represent fertilization of the female gamete by the male gamete. The nature of the body in which the head of the male gamete is embedded in 1 is not clear.
3—8. Oökinetes in smears of the contents of stomach of *A. maculipennis* twelve and a half hours after feeding on crescent case.
9. Early encysted zygote of *P. falciparum* in section of stomach. The single nucleus with large central karyosome is clearly shown. The cytoplasm is becoming vacuolated.
10. Slightly later stage than that depicted at 9. There has been some increase in size and the nucleus is in process of division. *P. falciparum.*
11. Surface section of a still later stage showing one of the daughter nuclei dividing in the cytoplasmic reticulum. *P. falciparum.*
12. Section through the middle of a cyst in about the same stage of development as 11. Irregular nuclei are scattered on the nodes of the now very much reticulated cytoplasm. *P. falciparum.*
13. Section of cyst with irregular nuclei. There has been a great increase in the chromatin material, which is arranged as karyosomes at the centre of the nuclei. *P. falciparum.*
14. Section of cyst with nuclei in various stages of division. The nucleus elongates while the karyosome becomes dumb-bell shaped and finally constricted into two parts. *P. falciparum.*
15. Section of cyst with more numerous nuclei, some of them dividing, arranged at the thickened nodes of the cytoplasmic reticulum. *P. falciparum.*
16. Section of cyst in which nuclear multiplication is practically completed. The cytoplasm is in the form of a sponge-work with thickened nodes. No separate sporoblasts have been found at any time. *P. falciparum.*

Plate II.—Figures drawn to the scale shown at 5, except 1, which is half the magnification of the others.

1—4. Tissue fixed in Schaudinn's fluid and stained with Mayer's acid hæmalum.
5. Fixed in Schaudinn's fluid and stained with iron hæmatoxylin.
1. Section of stomach of *A. maculipennis* which was fed on *P. vivax* case twice with a week's interval between the feeds. Large mature cyst containing sporozoites and residual bodies and two immature cysts containing irregular nuclei.
2. Section of mature cyst of *P. vivax* showing sporozoites and residual bodies.
3. Section of cyst of *P. falciparum* showing sporozoites which have been formed by outgrowth from the cytoplasm.
4. Section of cyst of *P. falciparum* showing sporozoites commencing to form as outgrowths from the surface of the cytoplasmic reticulum. Each outgrowth carries a nucleus with it to form the nucleus of the sporozoite.
5. Sporozoites of *P. falciparum* from salivary gland fixed in Schaudinn's fluid without drying and stained with iron hæmatoxylin. The nucleus has a central karyosome.

Plate III.—Tissue fixed in Schaudinn's fluid and stained with Mayer's hæmalum and eosin. The entire thoracic contents of the mosquitoes were dissected out, fixed and cut in serial sections.

Longitudinal section of salivary gland of *A. maculipennis* with sporozoites of *P. falciparum.* The drawing has been compiled from six serial sections.

Plate IV.—Tissue prepared as described for Plate III.

1 and 2. Longitudinal sections of lobe of salivary gland of *A. maculipennis* with sporozoites of *P. falciparum.*
3. Transverse section of two lobes of salivary gland of *A. maculipennis* with sporozoites of *P. falciparum.* In the upper lobe the secretion is within the cells and the duct is small, while in the lower lobe the duct is very much dilated by the secretion, in which the sporozoites are embedded. The cells are flattened.

Plate V.—Dried smear (Leishman stain) of the salivary gland of *A. superpictus* found naturally infected in Lahanah village. A single large gland cell is shown and the enormous number of sporozoites in such an infected mosquito is well illustrated.

NOTES ON THE INTESTINAL PROTOZOA OF 971 MEN AT THE UNIVERSITY WAR HOSPITAL, SOUTHAMPTON.

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During the year March, 1918, to March, 1919, I was engaged in the routine protozoological examination of the stools of dysenteric and other patients at the University War Hospital, Southampton, on behalf of the Medical Research Committee. Largely owing to the sympathetic interest of Lieutenant-Colonel R. E. Lauder, R.A.M.C., late Officer Commanding the University War Hospital, and to the excellent facilities very kindly placed at my disposal by Captain Wm. Fletcher, R.A.M.C., late pathologist to the University War Hospital, I was able to examine 971 patients with great care and thoroughness: and it seems worth while briefly to record the findings in the following communication.[1]

The 971 patients were made up as follows:[2]

527 were in hospital with dysentery.
210 „ „ „ other intestinal complaints.[3]
2 „ „ „ liver abscess.
95 „ „ „ other ailments.[4]
137 had been returned from the dysentery depot at Barton-on-Sea with a variety of complaints.

Of the 210 cases suffering from "other intestinal complaints," 51 had a history of dysentery, and 159 no such history. Of the 95 non-intestinal cases 25 had a history of dysentery and 70 had no such history.

Most of the men had been invalided from the various war-fronts, a few from stations in England[5]; 579 were invalided from Western Europe,

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[1] Some of these who were found to be infected with *Entamœba histolytica* subsequently underwent treatment with emetine bismuthous iodide under the most favourable conditions obtainable in a general military hospital. The results of treatment form the subject of a separate report.

[2] Two cases were undiagnosed. Three of the Barton cases were returned after discharge from the University War Hospital and are also included in the earlier number.

[3] Including diarrhœa, colitis, enteritis, melæna, constipation, typhoid and paratyphoid.

[4] Including pyrexia of unknown origin and trench fever, heart disorders, debility, malaria, hydrocele, myalgia, gunshot wounds, contused back, contused abdomen, influenza, pleurisy, bronchitis, pneumonia, tonsillitis, abscess of abdominal wall. These cases had all shown symptoms of intestinal disorder at some time.

[5] Very little significance can be attached to the place whence any man came to Southampton. Very often the men had stopped at several stations on their way, and in some instances spent many months in hospital there. The numbers here given are as nearly as possible correct for the last place where a stay was made, and I believe there is not more than a small error in any of them. The numbers for cases who had previously been in the East or the tropics should be quite accurate.

including 524 from France, of whom 133 had previously been in the tropics or the East; 52 from English stations, of whom 18 had previously been in the tropics or the East; and the rest from Ireland, Belgium, and Germany; 216 came from the Mediterranean area, including 69 from Italy, of whom 10 had previously been in the tropics or in the East, and 125 from Salonika; the others from Malta, Macedonia, Gallipoli, and the Balkans. 167 had been sent home from the East or the tropics, including 61 from Egypt, 30 from East Africa, 35 from Mesopotamia, 21 from Palestine, and the others from India, South Africa, Persia, and New Guinea. Two cases came from Canada, one from Mexico, and one from U.S.A. There is unfortunately no record of the place whence the four remaining men came.

It had been arranged in the time of my predecessor, Dr. D. L. Mackinnon, that six examinations should be made before a man was reported as being uninfected. This standard was well maintained in the present series. Altogether

777	cases	were	examined	6 or more times.
28	,,	,,	,,	5 times.
30	,,	,,	,,	4 ,,
48	,,	,,	,,	3 ,,
37	,,	,,	,,	2 ,,
50	,,	,,	,,	once.

More than half of those with fewer than six examinations were men found to be infected with *E. histolytica* at earlier examinations, but not at the University War Hospital.

1.—Infections.

The total infections discovered in the 971 cases are shown in Table I.

TABLE I.

Protozoa	No. of infected cases	Percentage of 970 cases
Entamœba histolytica	230	23·7
E. coli	295	30·4
Endolimax nana	278	28·6
Iodamœba bütschlii[1]	30	3·1
Dientamœba fragilis[2]	10	1·0
Giardia intestinalis	128	13·2
Chilomastix mesnili	63	6·5
Trichomonas hominis	12	1·2
Tricercomonas intestinalis[3] .	1	..

Altogether 63·4 per cent of the cases examined were found to be infected with one or more intestinal protozoa.

[1] "I-cyst," Wenyon. (See below.)

[2] See Jepps and Dobell, 1918.

[3] See Wenyon and O'Connor, 1917.

The eggs of *Ascaris lumbricoides* were found in 46 cases, or 4·7 per cent of the total number examined: and of *Trichuris trichiura* in 146 or 15 per cent of the cases. One patient, a native of Jamaica, harboured *Strongyloides stercoralis*[1] and an Ankylostome. It will be observed that the percentages are high, especially for the infections with *E. histolytica.* I can offer no explanation of the fact that these are so much more numerous in my figures, as compared with the published results of other workers, in proportion to the infections of other intestinal protozoa. Their detection was, of course, the primary object of the examinations, and it may be to this fact that their relatively high frequency in my series is due.

2.—The *E. histolytica* Infections.

This is, I believe, the highest frequency so far observed in a fairly large series of cases in a home hospital. I attribute this to the comparatively large number of very careful examinations it was possible to give to most of the cases.

It will be noticed that my predecessor at Southampton has recorded a much lower percentage (12·4) of men infected with *E. histolytica* (Mackinnon, 1918). In analysing her findings Dr. Mackinnon points out that a large proportion of her cases were non-dysenteric, and a large number had never been in places where amœbic dysentery is a common complaint. As regards the first point the two series are approximately equal—although there is a slight advantage on my side. My results are summarized in Table II.[2]

TABLE II.

Diagnosis	Number of cases	Percentage infected with *E. histolytica*	History of dysentery		No history of dysentery	
			Number of cases	Percentage infected with *E. histolytica*	Number of cases	Percentage infected with *E. histolytica*
Dysentery	527	26·5	..	..	..	..
Other intestinal complaints	210	19·5	51	19·6	159	19·5
Liver abscess	2	50·0	2	50·0	—	—
Non-intestinal complaints	95	23·2	25	32·0	70	20·0

There is, however, a considerable difference between the numbers of men who have never been in the East or the tropics in the two series: and this appears to be the more important factor of the two. In Dr. Mackinnon's series about seventy-five per cent of the cases had never been in the tropics or the East, whereas in mine only about fifty per cent of the cases are non-Eastern ones. In my series:—

[1] I am indebted to Captain Fletcher for this diagnosis.

[2] Columns 3-6 give a fuller analysis of the cases in column 1.

In 494 non-Eastern cases there were 106 infected with *E. histolytica*, or 21·5 per cent.

In 478 Eastern cases there were 124 infected with *E. histolytica*, or 26·1 per cent.

So that my percentage of infected men should be the higher. But it will be noticed that my percentage (21·5) for *non-Eastern* cases is actually higher than the percentage (16·1) for the *Eastern* cases in the earlier report. The difference, therefore, is not in any high degree due to this dissimilarity between the two groups of cases. The important point of difference lies in the much smaller number of cases examined by me.[1] I am convinced that the greater length of time thus available for each case, and consequently the more thorough examination possible, is the chief differential factor. It is possible that an additional year of active service may have increased the intestinal infections of the men in the Army—at any rate of those who had been in the East—to an appreciable extent: but I know of no data which can supply any information on this point.

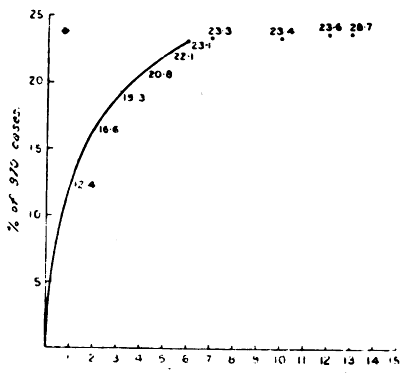

FIG. 1.—Showing the increased percentage of infected cases at successive examinations.

The total percentage (23·7) of cases found to be infected with *E. histolytica*, it is interesting to note, is very near the theoretical twenty-three per cent calculated by Carter, Mackinnon, Matthews and Malins Smith (1917) as the actual proportion of infected cases in a series of military patients at Liverpool suffering from intestinal complaints, and the curve shown in fig. 1 may be compared with their fig. 1 which shows the *calculated* rise in the percentage of infected cases at several examinations. My fig. 1 shows the actual increase in this series of 971 cases. At each succeeding examination, of course, fewer cases are examined, but in this series only eighty *negative* cases have fallen out between the first and the sixth examination; rather more than half the total falling off (193) is due

[1] Dr. Mackinnon actually examined 1,680 men in 10½ months.

to the fact that cases already found positive are omitted from the later examinations. So that up to the sixth examination the curve should correspond fairly well with the theoretical one. Beyond the sixth examination many of the negative cases are dropped, so that the later points are of very little value.

It is perhaps of greater interest that the final percentage of infected cases in this series (23·7 per cent)[1] lies within the limits given by Dobell (1917) for the probable actual incidence of *E. histolytica* in "carrier" cases, as his numbers (eighteen to twenty-five per cent) are rather more securely founded on the actual findings in series of examinations. (The Liverpool figure is also within these limits.)

Table III gives the actual percentage of infected cases at the several examinations.

TABLE III.

Examination	Cases infected with *E. histolytica*			
	Number of cases in which were found			Percentage of 971 cases infected
	Cysts over 10 μ in diameter	Only cysts less than 10 μ in diameter	Amœbæ only (no cysts)	
1	77	34	9	12·4
2	12	23	5	16·6
3	13	13	1	19·3
4	7	7	1	20·8
5	5	7	..	22·1
6	2	8	..	23·1
7	..	2	..	23·3
10	1	..	..	23·4
12	..	1	1	23·6
13	1	..	..	23·7

It will be noticed that the greater difficulty of discovering the small-sized cysts is reflected in the more even distribution of the numbers at the early examinations in the third column.

Of the 230 cases infected with *E. histolytica*, 99 cases passed only cysts greater than 10 μ in diameter; 95 cases passed only cysts less than 10 μ in diameter; 19 cases passed cysts above and below 10 μ in diameter.

It will be noticed that there are approximately equal numbers of cases infected with strains forming cysts of large (above 10 μ) and small (below 10 μ) size respectively.

In her report of September, 1918, Dr. Mackinnon notes that a larger proportion of her positive cases passing large-sized cysts had been "in the

[1] If the fullest allowance be made for the eighty negative cases which dropped out before the sixth examination the percentage is not above twenty-five.

[2] In the seventeen remaining cases no cysts were ever seen.

East" than of those passing only small-sized cysts—suggesting that strains of *E. histolytica* forming large-sized cysts are commoner in the East. In the present series there is the same indication: Of 99 cases passing only large cysts 64 had been in the East; of 95 cases passing only small cysts 36 had been in the East; and of 19 cases passing large and small cysts 10 had been in the East.

Although a fair number of infections with small cyst-producing strains of *E. histolytica* have now been studied by different workers (see Dobell and Jepps, 1918), very little[1] has been said about the corresponding tissue-invading form of the amœba. Cutler (1919) reports that he infected a cat by feeding it with cysts of a small size; the cat developed dysentery and died, and "a small variety of tissue-invading amœbæ" was found in its intestine. Other observations appear to confirm the pathogenicity of those small cyst-forming strains; but it seems a little curious that the corresponding "*histolytica*" amœba has yet to be properly described. In the present series of 230 infected men twenty-two were examined on days when they were suffering from acute or sub-acute dysentery and large *histolytica* amœbæ found in all of them. Eleven of these cases subsequently passed cysts, and they were all over 10 μ in diameter. I would venture tentatively to suggest that small cyst-producing strains of *E. histolytica* may perhaps be unlikely to produce severe dysenteric symptoms in the human host as frequently as the strains forming cysts of the larger sizes. Further observations on the *histolytica* stage of small cyst-forming strains are very desirable and may not harmonize at all with my suggestion. Inferences drawn from a series of cases such as these are rendered uncertain by the fact that many men had received previous treatment with emetine.[2]

3.—The Barton Cases.

During the year there were 137 cases sent to the University War Hospital from the dysentery depot at Barton-on-Sea. These were all men originally invalided for dysentery, who had been sent to Barton from their hospitals as convalescent, and fit for training up to their full strength. Only a small number of these (6) were sent back to hospital on account of recurrent dysentery, and a consideration of the returned depot cases by themselves may prove of interest.

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[1] For a summary, see Dobell (1919), p. 55.

[2] It is interesting, also, to note an indication that strains of *E. histolytica* forming small cysts may be more easily removed by emetine treatment than strains forming cysts of 10 μ or more in diameter. Of 58 men passing cysts of known size who were treated with a twelve-day course of emetine bismuthous iodide, 24 passed cysts below 10 μ in diameter only, 34 passed cysts above 10 μ in diameter. Al of the former were discharged as cured at the end of at least four weeks' observation after the completion of treatment, while eight of the latter relapsed, five of these being acute or subacute cases. It is hardly possible that this difference is insignificant or due to failure to detect relapses in the cases passing small cysts only. Only two of the relapsed cases had records of previous full courses of emetine treatment.

Besides 6 cases suffering from dysentery, there were: 1 case suffering from hepatitis; 7 cases suffering from post-dysenteric diarrhœa or debility; 36 cases suffering from intestinal disorders[1]; 22 cases suffering from influenza, pneumonia, bronchitis, etc.; 17 cases suffering from malaria; 11 cases suffering from gastric troubles; 37 cases suffering from other complaints.[2]

Five patients were sent with a diagnosed infection of *E. histolytica*; of these one was said to be suffering from amœbic dysentery, one from hepatitis, two from diarrhœa. One patient was sent as a carrier of *B. dysenteriæ* Flexner. For the others the examinations for the organisms of dysentery were declared "complete and negative" in 102 cases, and "incomplete" in twenty-nine cases.

At Southampton twenty-five of these men were found to be infected with *E. histolytica*, including the five already found at Barton, i.e., 18·3 per cent. Omitting these five, 20 cases of the remaining 132, or 15·2 per cent, were found to be infected with *E. histolytica*. Of the 102 "complete and negative" cases 11 were found to be infected with *E. histolytica* or 10·8 per cent. Six cases were in a subacute condition when they were examined, passing blood, mucus, and *histolytica* amœbæ; and only seven appeared to have no symptoms attributable to *E. histolytica*. One of these latter cases was the man who came in with a diagnosis of amœbic dysentery. He had a large infection of *E. histolytica*, although his stool was quite normal at the time when it was examined. So that probably at least seven of the men were suffering considerable inconvenience from their infections.

Of the 25 positive cases: 11 passed only cysts greater than 10 μ in diameter; 4 passed only cysts less than 10 μ in diameter; 4 passed cysts above and below 10 μ in diameter: 6 passed only amœbæ.

Only one case was examined a very large number of times before the infection was discovered; 12 infections were detected at the first examination; 6 at the second examination; 4 at the third examination; 1 at the fifth examination; 1 at the sixth examination; 1 at the twelfth examination. Eighteen cases were, therefore, discovered to be infected by the first two examinations: only eight of these, however, were sent from Barton as completely examined and found negative, i.e., 7·8 per cent of the 102 cases. This figure compares favourably with the twelve per cent found to be infected by Dobell (Dobell, Gettings, Jepps and Stephens,

[1] Including diarrhœa (8), colitis (9), hæmorrhoids (5), hæmorrhage (4), hernia (3), constipation (1), appendicitis (2), melæna (1), incontinence of fæces (1), sigmoid ulcer (1), ? enteric (1).

[2] Including rheumatism, neuritis, neurasthenia, anæmia, debility, tachycardia, photophobia, pyrexia, nephritis, urethral stricture, albuminuria, bacilluria, hæmorrhage from throat, loose knee cartilage, elbow injury, fractured femur, synovitis, abscess, cystitis orchitis, cholecystitis, pyrexia of unknown origin, anal fissure.

1918), amongst convalescent dysenterics at the depot at Epsom. The percentage of positives among these men who had passed the depot stage, though still high, is considerably lower than that found for the Epsom cases still in the depot.

That so large a proportion of men previously examined for *E. histolytica*, both in hospital and at the dysentery depot, should have escaped detection is further evidence of the ease with which the infection may be missed: and further proof of the necessity of special arrangements formed to meet this difficulty. When such arrangements are made a fair proportion of the infections are probably discovered: the majority of the dysenteric patients from the University War Hospital went to Barton-on-Sea on their discharge from Southampton, and during the whole year only one of these was reported positive from Barton after one or two further examinations.

4.—The I-Cyst, Wenyon 1916.

Throughout this series of examinations the structure known amongst workers in this field as the "I-cyst" of Wenyon, or the I-body, has been recorded, and careful observations always made in the hope of finding some other stage of the organism. In June and July of 1918, two cases from whom I had obtained I-bodies produced some amœbæ of a kind new to me: a study of these, and of some intermediate forms present in the stools led me to believe that they were the organism of which the I-body is in fact the encysted stage.

Mr. Clifford Dobell, who had previously been making observations on the I-body for some time, and had very kindly told me about them, has seen some of my stained preparations, and agrees with me that my amœba is the organism forming the I-body as its cyst. Mr. Dobell (1919) has since published a full account of the amœba and its cyst, where the references in previous literature are given, and the new generic name *Iodamœba* is proposed for it. The correct name for this organism is now *Iodamœba bütschlii* (Prowazek, 1912), Dobell, 1919.

Iodamœba is a true intestinal parasite, and dies very rapidly after leaving the human body. Degenerate forms are not uncommon in diarrhœic stools from patients infected with the amœba, but could hardly be recognized unless the normal amœba were known. There is no contractile vacuole: and the amœba resembles *Entamœba coli* very closely in the living state in its appearance, in its movements, and in the contents of its food vacuoles. The most obvious differences are its slightly smaller average size, and the fact that no nucleus is as a rule visible in the living *Iodamœba*. In fixed and stained preparations, of course, the amœba is readily distinguished from *E. coli* by its very different nuclear structure. In these preparations the nucleus at first sight bears a resemblance to that of *Endolimax nana*: and this resemblance, though only superficial, appears to have led to some confusion of these two organisms. Kofoid,

Kornhauser and Swezy (1919), for example, have recently published an account of *E. nana*, in which they describe the existence of "large" and "small races." It is very probable that races producing larger and smaller cysts respectively do exist in this species, as in the other human intestinal protozoa; but the description given by these three authors of their "large" race indicates that it consists for the most part of *I. bütschlii*. Perhaps the most significant point in this connexion is their statement that "two nuclei are rarely found in these larger cysts, and, as yet, we have not found four in them." These resemblances do not, however, prevent the certain distinction of an infection of *Iodamœba* from any of the other amœbæ described as occurring in the human intestine; although, of course, a single individual might be difficult or impossible to diagnose with certainty.

The only point of interest regarding the distribution of this organism shown by my figures is its frequent occurrence with *Entamœba coli*, *E. histolytica*, or *Endolimax nana*, and particularly with *E. histolytica*. Out of 777 men examined six times each, there were [1] 118 men infected with *E. histolytica*, and 20 men with *Iodamœba*. The following figures show how the *Iodamœba* infections were distributed:—

Of 118 men infected with *E. histolytica*, 10 also had *Iodamœba*, or 8·5 per cent. Of 659 men not infected with *E. histolytica*, ten also had *Iodamœba*, or 1·5 per cent.

I have never found a case to be infected only with *Iodamœba* after thorough examination, and there is usually an infection of *Entamœba* or *Endolimax* along with it (cf. Dobell loc. cit.).

It has been observed more than once that infections of *I. bütschlii* have disappeared after a course of emetine treatment for a concurrent infection of *E. histolytica* (see Dobell loc. cit.). At the University War Hospital six men with infections of both *E. histolytica* and *I. bütschlii* have had courses (36 grains) of emetine bismuthous iodide, and neither organism has again been seen in at least twelve examinations during four weeks after the end of treatment. In the absence of any record of prolonged examination of untreated cases, it is perhaps not possible, as Wenyon and O'Connor (1917) have observed, to attribute this disappearance of *I. bütschlii* to the action of the emetine with certainty, but the evidence is accumulating against a series of coincidences. It is very interesting if emetine is indeed an infallible cure for an infection of *I. bütschlii*, as Dobell has pointed out, in view of the fact that the amœba apparently lives on bacteria and is not a tissue parasite; for it is supposed (see Dale and Dobell, 1917) that emetine has no direct action on *E. histolytica* and only removes it by affecting the tissues of the host in some way prejudicial to the parasite.

[1] This number would probably have been higher if none of the men had received emetine treatment before the sixth examination.

ACKNOWLEDGMENTS.

It gives me the greatest pleasure to place on record my sincere thanks to Lieutenant-Colonel R. E. Lauder for his most helpful interest throughout the year; this work could not have been carried out without the support of his ever-ready attention to detail, and his skill in organization. To Captain Wm. Fletcher my best thanks are due for his exceedingly valuable interest, suggestions, and criticism; as well as for the very kind way in which he allowed me to make the fullest use of his excellent laboratory and staff. Once again my thanks are due to Mr. Clifford Dobell for much valuable advice and criticism. It is a pleasure to offer my thanks again to the Wellcome Bureau of Scientific Research for their continued hospitality during the preparation of this report.

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Clinical and other Notes.

BILHARZIASIS AMONGST BRITISH TROOPS IN MESOPOTAMIA.

Notes on the Treatment of Thirty-one Cases by Intravenous Injections of Tartar Emetic.

By Captain A. G. HARSANT.
Royal Army Medical Corps.

The ova of *Schistosoma hæmatobium* were found in all these cases, which occurred amongst the British Garrison on the Euphrates during the winter of 1920-1921. Twenty-six of these cases were from the Royal Ulster Rifles, five from the 19th Brigade Royal Field Artillery.

Place of Infection.—Consideration of the movements of the affected men indicate that infection occurred around the neighbouring garrison of Kufa and Kifl, probably through contact with infected water, in baths filled direct from the River Euphrates.

Incubation Period.—In twenty-four of the men the symptoms commenced from six to fifteen weeks after their arrival in the Kufa area. Three, however, had no symptoms till twenty-three weeks after arriving at Kufa.

Symptoms.—Twenty-nine cases reported sick with urethral pain on micturition and hæmaturia; two cases reported on account of the passage of blood with their stools and were admitted to hospital as "piles" and "dysentery" respectively. In some cases the symptoms were continuous, in others intermittent, with quiescent periods of from one to four weeks. Two cases (in whose stools ova were not found on admission to hospital) stated that they had passed blood and mucus with their stools before any vesical symptoms arose.

Examination of Urine.—In one case the ova were only found in the urine on the fifth examination. In the remainder pus and ova, and frequently blood, were easily demonstrated.

Examination of Fæces.—The two cases with rectal symptoms showed formed fæcal stools with bright blood and mucus externally; ova were found easily in the mucus and, in scanty numbers, in the substance of the stool.

In addition, scanty ova were demonstrated in the substance of the stool in three other cases whose symptoms were solely vesical, and whose stools showed no blood or mucus. The ova of *S. hæmatobium* were alone found, both in vesical and rectal cases; no lateral spined ova were found, after careful search, in the urine and stools of all cases.

Examination of Blood.—In all cases there was a leucocytosis, with a relative increase in the eosinophiles and a rather diminished percentage of polymorphonuclears.

Treatment.—The interval between the onset of symptoms and the commencement of specific treatment ranged from five days to seven weeks.

Case 1.—Was treated at first with daily hypodermic injections of emetine hydrochloride one grain for eight days. This had no effect on his symptoms, or

on the amount of pus or ova in his urine. Thereafter treatment of all cases was with intravenous injections of tartar emetic, well diluted with saline, given by gravity through funnel and tubing. A bland mixture containing citrates and hyoscyamus was given throughout the course. The tartar emetic ½ grain was dissolved in one cubic centimetre distilled water, sterilized in the autoclave and stored in rubber-capped bottles. The injections were given every alternate day. The required quantity of the tartar emetic solution having been taken up in a sterile syringe the median basilic or other prominent vein was punctured with the

TABLE I.

	Before treatment						After treatment					
No. of case	Total lencocytes per c.mm.	Poly-morphs.	Lympho-cytes	Large mono-nuclears	Eosino-phil	Baso-phil	Total leucocytes per c.mm.	Poly-morphs.	Lympho-cytes	Large mono-nuclears	Eosino-phil	Baso-phil
1	19,200	35	28	6	31	—	—	—	—	—	—	—
2	9,880	39	52	6	3	—	—	42·5	47	4·5	5	—
3	10,058	50	31	6	12	—	—	38	39	6	7	—
4	13,700	36	42	6	15	—	—	42	35	2	21	—
5	13,300	45	38	7	10	—	—	32	37	8	23	—
6	9,760	51	37	1	11	—	—	35	44	4	16	1
7	9,760	56	30	4	10	—	—	34	39	5	22	—
8	12,800	48	36	7	9	—	—	45	27	14	14	—
9	10,800	52	32	4	11	1	—	33	50	2	12	3
10	11,280	52	30	4	14	—	11,320	42	35	2	20	1
11	8,520	62	31	4	3	—	8,400	66	24	2	8	—
12	10,240	52	31	2	15	—	16,000	36	28	—	36	—
13	12,960	53	31	4	12	—	11,840	45	36	—	19	—
14	12,320	37	42	6	14	1	—	—	—	—	—	—
15	8,160	47	45	—	8	—	9,120	34	55	3	8	—
16	12,480	42	36	2	19	—	14,080	33	36	2	29	—
17	12,560	62	30	—	8	—	—	—	—	—	—	—
18	16,640	59	30	3	8	—	—	—	—	—	—	—
19	9,760	70	13	5	12	—	—	—	—	—	—	—
20	10,320	49	39	6	5	1	10,040	54	33	1	12	—
21	9,040	58	31	5	6	—	8,320	38	38	3	20	1
22	6,560	48	40	5	6	1	—	—	—	—	—	—
23	11,040	39	36	2	22	—	10,560	38	28	3	31	—
24	7,440	50	32	3	14	1	12,400	40	43	8	9	—
25	7,681	39	36	2	23	—	13,200	23	29	2	46	—
26	12,560	45	41	2	12	—	9,400	40	34	3	23	—
27	12,240	50	32	2	16	—	—	30	28	6	34	2
28	7,680	56	25	2	17	—	—	—	—	—	—	—
29	14,800	50	35	4	11	—	—	—	—	—	—	—
30	11,360	26	49	5	18	2	—	—	—	—	—	—
31	11,120	48	31	3	18	—	—	—	—	—	—	—

needle of the gravity apparatus—saline was allowed to flow into the vein, to ensure that the needle was properly within the lumen—and the dose of the drug was then squirted into the saline in the funnel (about fifty cubic centimetres). Finally this was washed in with a further twenty cubic centimetres of saline.

Dosage.—In the first few cases, the preliminary dose was ½ grain which was increased by ½ grain on alternate days, till 2½ grains were given at a dose. Later, however, the first injection was one grain, and subsequent injections were increased by one grain up to three grains at each injection; this dose was continued every alternate day unless contra-indications arose, till thirty to thirty-five

TABLE II.—SUMMARY OF RESULTS.

No. of case	Date of onset of symptoms	Date of commencement of treatment	Ova present in: Urine	Ova present in: Fæces	Total number of grains given	Number of doses	Whether ova were present on discharge	Number of days after end of treatment that urine was re-examined	And if ova still present	Remarks
1	14.12.20	6.2.21	+	—	30¼	15	—	57	—	In hospital eight weeks before specific treatment started. Given hypodermic emetine hydrochloride 1 gr. for eight days; no benefit; four days after end of tartar emetic treatment, slight irregular pyrexia, with some urticaria of eyelids and arms
2	31.12.20	6.2.21	+	—	30¼	15	—	57	—	—
3	2.1.21	6.2.21	+	—	30¼	15	—	57	—	Slight conjunctivitis seven days after end of treatment, lasting one week
4	27.1.21	6.2.21	+	—	30¼	15	—	59	—	Pallid, and alleged to have had D.A.H. recently; no complications during treatment
5	26.1.21	14.2.21	+	—	31	14	—	47	—	—
6	18.1.21	20.2.21	+	—	31	14	—	45	—	Slight conjunctivitis after 28½ gr.; herpes zoster along course of 7th and 8th thoracic nerves seven days after end of treatment
7	10.1.21	20.2.21	+	+	31	14	—	45	—	Rectal symptoms; admitted as "piles"; also some vesical symptoms
8	11.2.21	16.2.21	+	+ Scanty	30½	14	—	14	—	Otitis media developed after 16½ gr., lasting for ten days; stated he had never had any previous ear trouble
9	28.12.20	24.2.21	+	—	32	14	—	41	—	—
10	28.2.21	2.3.21	+	—	32	14	A few granular ova	35	—	—
11	14.2.21	4.3.21	+	—	32½	14	—	33	—	Ova only found in urine on fifth examination.
12	14.2.21	4.3.21	+	—	32	14	—	33	—	Otorrhœa (right) after 14½ gr.; extensive herpes along course of cervical 3rd and 4th spinal nerves, and posteriorly along course of 12th thoracic, at end of treatment
13	10.2.21	12.3.21	+	+	31	13	—	30	—	Rectal symptoms; admitted as dysentery; also vesical symptoms

14	28.12.20	18.3.21	+	–	30	12	–	2	–	Pain in right loin and along course of right ureter on admission, which disappeared after six doses; blepharitis after 21 gr.
15	26.12.20	12.3.21	+	+ Scanty	31½	13	–	29	–	Second injection partly extravenous; suppuration; healed well after incision and daily massage
16	20.2.21	18.3.21	+	–	30	12	–	26	–	Bilateral otorrhœa after 30 gr. History of previous attacks
17	25.3.21	3.4.21	+	–	31½	13	–	2	–	Chronic ulcer (Leishman-Donovan bodies not found) in skin; healed completely after 25 gr.
18	28.3.21	3.4.21	+	–	34½	13	–	3	–	Vomiting rather marked after injections
19	30.3.21	3.4.21	+	–	29	12	–	4	–	,, ,, ,, ,,
20	8.1.21	9.4.21	+	+ Scanty	35½	13	–	4	–	–
21	9.4.21	14.4.21	+	–	33	12	–	2	–	–
22	7.1.21	17.4.21	+	–	24½	10	–	3	–	Much vomiting, with abdominal pain, diarrhœa and collapse, after the larger doses
23	12.4.21	19.4.21	+	–	32½	12	–	4	–	–
24	12.4.21	19.4.21	+	–	32½	12	–	4	–	–
25	10.1.21	29.4.21	+	–	31	13	–	4	–	After 26 gr. urine showed a cloud of albumin and granular casts; not found again
26	20.1.21	27.4.21	+	–	33½	14	–	5	–	–
27	18.1.21	22.2.21	+	–	32	14	–	8	–	Definite pain in shoulders after injections; urine free from ova on discharge
	Relapse 15.4.21	29.4.21	+	–	7½	3	–	4	–	Relapsed; scanty ova again present. Refused further treatment after 7½ gr.
28	26.1.21	23.4.21	+	–	18	7	–	3	–	
29	20.2.21	27.4.21	+	–	12	5	–	3	–	
30	14.2.21	27.4.21	+	–	12	5	Ova present	3	Ova present	Refused to complete treatment
31	10.1.21	1.5.21	+	–	1	1	Ova present	–		

grains had been injected. The injections required were from twelve to fourteen, the patient thus being under treatment for about a month.

Effects of Treatment.—(a) Immediate. With the smaller dose, nil. The larger doses were followed, at the end of the injection, by a short paroxysm of coughing with expectoration or by nausea and vomiting. The vomiting was more pronounced towards the latter part of the course but varied much in different cases.

One case (No. 22) had marked symptoms after the sixth dose, when a total of fifteen grains had been administered. The patient was seized with vomiting, diarrhœa, and abdominal pains, and became rather collapsed. These symptoms completely disappeared in twenty-four hours and subsequent smaller doses were well borne. The second dose given to Case 15 was administered with a twenty cubic centimetre syringe instead of by gravity; some of the solution escaped extravenously resulting in considerable induration around the anti-cubital fossa. The area suppurated two days later, but after incision, followed by vigorous daily massage, healed well, leaving no limitation of movement.

(*b*) *Delayed.*—Many cases complained of general influenza-like and muscular pains on the day following the injection, which disappeared within twenty-four hours. Some patients complained of rather severe pain in the shoulder and deltoid region. At first the patients were kept in bed throughout the course, but latterly were allowed up on the days on which they did not receive an injection.

Effect on the Blood.—Table I shows the differential count at the commencement and termination of treatment.

Complications.—Towards the end, or after the termination of their course—a number of cases developed minor complications—such as otorrhœa, conjunctivitis, blepharitis and herpes. Possibly this may be attributed to the effects of the drug upon the phagocytes, the percentage of which fell during treatment.

Results of Treatment.—The symptoms of pain and hæmaturia usually disappeared after five to six doses had been given. Pus and ova persisted in the urine for much longer periods. The first effect noticed on the ova, was a loss of translucency accompanied by a delay in hatching out. After further doses the ova became blackish, of a generally granular appearance and the contained miracidium could no longer be differentiated from the shell.

The amount of the drug which had been injected when ova, capable of hatching out, were last seen in the urine, varied from three to seventeen grains; in two cases, however, twenty-one and twenty-six grains respectively had been injected when such ova were last seen. The black granular ova persisted in some cases to the end, or almost the end of, treatment. Thirty-one cases were treated; of these, four refused to continue their treatment after they had received one to four injections. Of the remaining twenty-seven cases only one is known to have relapsed. Sixteen of these twenty-seven cases were still free from pus and ova in their urine when re-examined four to eight weeks after discharge from hospital.

On the termination of treatment, all cases were entirely free from symptoms, a few were passing occasionally a few black granular ova, but none showed any ova capable of hatching out.

The one case which is known to have relapsed, completed a full course of fourteen injections (thirty-two grains) and was discharged from hospital after

several negative examinations of urine. Shortly after discharge, he had a recurrence of symptoms whilst on a march; ova were found in his urine only on the sixth examination. He was given three injections of tartar emetic (7½ grains), but then refused further treatment.

Table II gives a summary of the treatment and results.

Table III records the subsequent examination of the urine of all men in the Royal Ulster Rifles and 19th Brigade Royal Field Artillery who had been in the infected area.

TABLE III.

Regiment	Number examined	Number whose urine showed		Total infected
		Numerous ova	Few ova	
R.U.R. ..	Officers .. 12	*Nil*	*Nil*	*Nil*
	Other ranks.. 481	29 (6 per cent)	82 (7·5 per cent)	111 (23·5 per cent)
19th Brigade R.F.A.	Officers .. 1	*Nil*	*Nil*	*Nil*
	Other ranks.. 35	*Nil*	11 (31 per cent)	11 (31 per cent)

There was no opportunity of searching the suspected area of infection for the snail hosts of the parasites as the area was evacuated by our troops before many cases were admitted to hospital and interest in the infection had been aroused.

It is regretted that the movement of the troops to Egypt has prevented me from following up and recording the final results of the treatment, and from treating the other 122 cases in whose urine ova were subsequently found, but these notes are published in the hope that the progress of the cases can be followed and recorded at a later date in the JOURNAL OF THE ROYAL ARMY MEDICAL CORPS. A nominal roll of these cases has been submitted to the Medical Authorities, Egypt, for this purpose.

I owe more than I can here express to the A.D.P. Mesopotamia, Lieutenant-Colonel Hamerton, C.M.G., D.S.O., R.A.M.C., for his help and suggestions throughout; it is due entirely to his persuasion that I have placed these few notes on record.

IODINE IN THE PROPHYLAXIS OF INFLUENZA.

By CAPTAIN D. W. BEAMISH.
Royal Army Medical Corps.

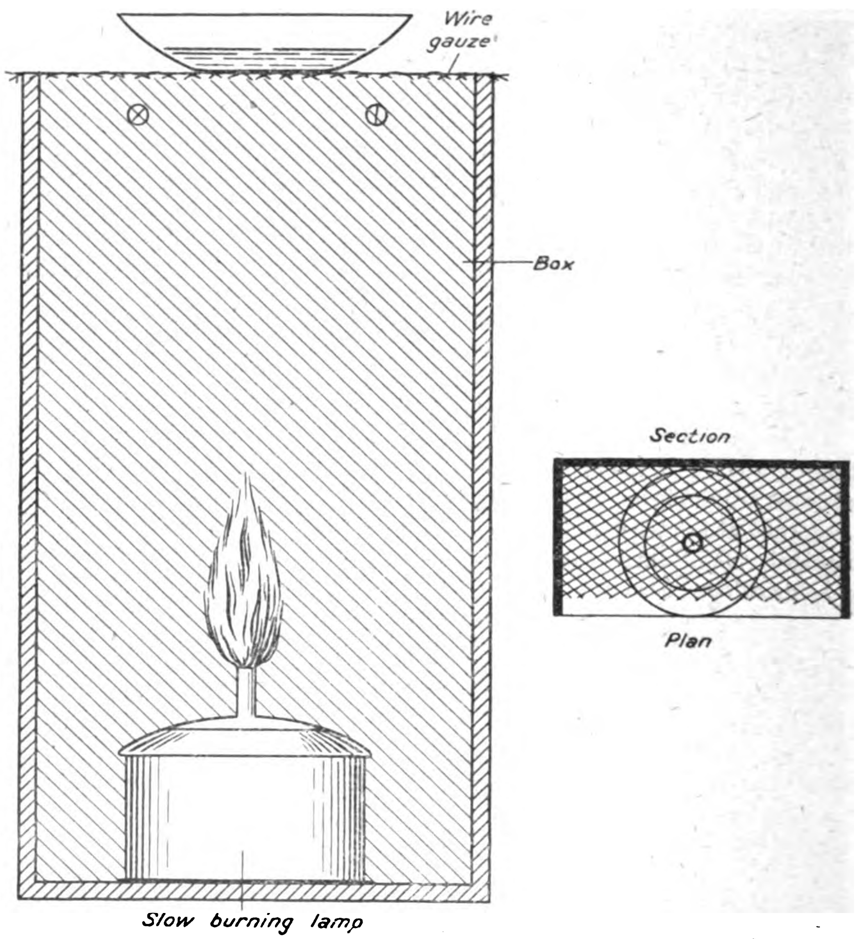
SAJOUS (1920) has emphasized as a result of his experiments and those of others, the importance of inhalation of fumes produced by heating pure iodine crystals, in the prophylaxis of influenza. This is recommended for trial in all public places, barracks, schools, etc.

I have carried out the following experiments in Kasauli and Sanawar and think it may be of interest to describe them in some detail. The experiments about to be described are based on the theory: (1) that we are protected to a very high degree by the natural nasopharyngeal defences and the defences of the respiratory tract; (2) that iodine fumes in suitable doses are particularly powerful

in stimulating these defences, which are thus enabled to deal successfully with an invasion of organisms such as the *Bacillus influenzæ*.

The doses and method of carrying out the experiment are as recommended by Sajous.

No. 1 at Kasauli Barracks.—At the time of carrying out this experiment in February, 1921, influenza was mildly prevalent amongst the European residents in Kasauli and so it was thought a favourable time to start the work in the barracks.



There were then two bungalows in occupation by the troops. The cubic capacity of the rooms having been ascertained, the amount of iodine crystals required was arrived at, bearing in mind the fact that 0·33 gramme is sufficient for a room of sixteen cubic metres capacity.

The cigar box method, being the most economical was the one employed.

The crystals were heated in the barrack-rooms at night for three hours commencing at about 10 p.m. The lamp was placed in the middle of the room.

I found that a small lamp of local manufacture which contained three ounces of methylated spirits lasted just three hours. The flame is placed about six inches below the saucer in position on top of the cigar box (see diagram).

The size and position of the flame can only be arrived at by visiting the rooms and altering as required.

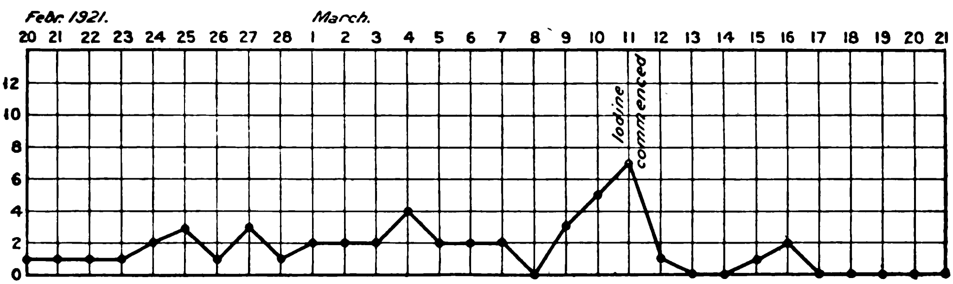
There should be a faint odour of iodine throughout the room and no excess of fumes which would cause any discomfort on the part of the men.

I found that a flame a little more than half the size of an ordinary candle flame did not produce any bad effects.

There were no complaints on the part of the men who had been previously lectured on influenza and I experienced no difficulty in carrying out the work.

The iodine was weighed out and the lamp refilled daily at the British Station Hospital. One man was detailed from each room to take his supply of iodine and methylated spirits from the hospital. The senior non-commissioned officer was made responsible for lighting the lamps before retiring to bed.

Cost of the Experiment.—The cost of the iodine which is 11 rupees a pound worked out at about 7 annas per night and the methylated spirits $1\frac{1}{2}$ annas per



Admission incidence for influenza.

night. This is the cost of iodine and spirits used in a total space of over 1,000 cubic metres, housing about three quarters of the men in the station at the time. The lamps, which are of scrap metal, only cost two annas each. The experiment was only carried on for about one month as influenza disappeared from the station. There were no cases of influenza amongst the troops at any time during the season.

Experiment 2.—A mild outbreak of influenza occurred in the Lawrence Royal Military School, Sanawar, during March, 1921. As the cases increased, in the usual course of events the classes would have to be stopped and the whole school thus disorganized. I suggested that a trial should be given to the iodine prophylaxis before closing down the schoolrooms. This was carried out for three hours during the morning classes in all the schoolrooms.

The results were interesting, the curve of admissions rapidly falling, and within a few days there were no admissions. Of course this could not have been entirely due to the iodine but the result is certainly interesting, as there were no more cases after six days' treatment. The presumption is that iodine must have had some part to play.

From a teaching point of view it was very satisfactory as the classes were

carried on as usual with no complaints from the teachers or pupils. In conclusion I think this form of prophylaxis is worth further trials in view of its simplicity and small cost.

I wish to thank Assistant Surgeon Aling of the British Station Hospital, Kasauli, and Assistant Surgeon Raj Singh, of Sanawar, for their valuable assistance in carrying out these experiments.

REFERENCE.

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A CASE OF BLACKWATER FEVER OCCURRING IN MESOPOTAMIA.

By CAPTAIN A. G. HARSANT.
Royal Army Medical Corps.

THE 2nd Battalion York and Lancs. Regiment, were heavily infected with malaria whilst stationed at Kasvin (Persia) during autumn of 1920 and spring of 1921.

Lance-Cpl. G., of that Regiment stated that he had sixteen attacks of malaria during that period, for ten of which he was admitted to hospital. He had been having ague attacks every three to four weeks and was discharged from hospital after the last of these, about April 10. He had taken much quinine by mouth and was given daily intramuscular injections of quinine for eight days in December, 1920. He was admitted to 23rd British Station Hospital, Baghdad, on April 25, 1921, apparently suffering from an ordinary malarial relapse. For two days he had an intermittent temperature. His colour and general condition were good, his spleen was enlarged considerably below the costal margin and tender, and he vomited at intervals. He was given a mixture of quinine fifteen grains, three times a day.

On the evening of the third day, he complained of pain in the right side of his chest, apparently superficial; no abnormal physical signs could be detected in his chest.

On the fourth morning patient was very collapsed, was vomiting frequently; his skin was slightly jaundiced and his urine was almost black. He was given an intramuscular injection of quinine bihydrochloride fifteen grains at 7.30 a.m. At 10.30 a.m. he had a severe rigor, temperature 103° in axilla. Pulse 136, very weak. He was vomiting bilious fluid and his jaundice was rapidly increasing. There was tenderness over the spleen and in the right hypochrondrium where the lower edge of the liver was just palpable. He was given a further intramuscular injection of quinine at 11 a.m. On the fifth day, the patient was pallid and collapsed, and was still vomiting. One pint of bicarbonate and saline, and ten grains quinine bihydrochloride were given intravenously. The bicarbonate and saline injection was repeated on the seventh day, when the patient's condition was still critical.

There was definite improvement after each of these injections: his temperature however rose to 104° on the seventh day.

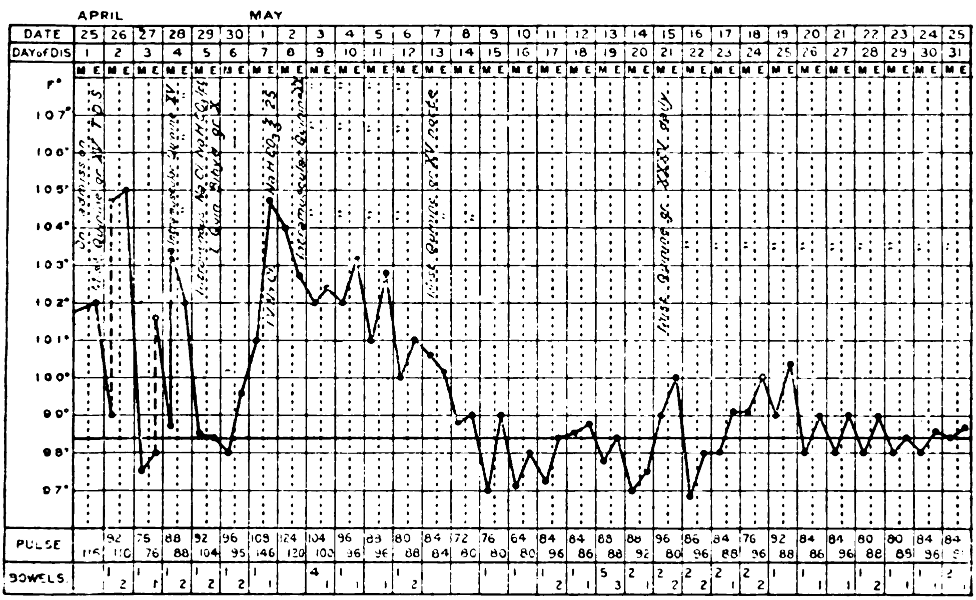
Although malarial parasites could not be demonstrated in his blood—he was given an intramuscular injection of quinine on five successive days. This resulted in a steady fall of temperature—and an equally steady improvement in symptoms.

The first few days he was given large quantities of alkaline drinks, with brandy and raisin tea; these for the greater part he retained. He passed from forty to sixty ounces of urine in twenty-four hours; hæmoglobin was not seen after the sixth day.

There was a friction rub heard over the spleen on the eleventh day—he complained of severe pain in this region for the following twelve days—and of flatulence and abdominal distension.

There were no abnormal physical signs at any time.

He steadily improved—was brought before a Medical Board and was evacuated to the base for transfer to England on the twenty-fourth day—May 28, 1921.



Four examinations of blood, both by thick and thin film methods, failed to show any malarial parasites (but the patient had been taking quinine before admission to hospital).

Blood Counts:—

	Red cells	Hæmoglobin
May 3, 1921	1·8 million per c.mm. ..	45 per cent of control blood
,, ,, ,, (repeated)	2·0 ,, ,, ..	,, ,, ,, ,,
,, 5, ,, ...	2 3 ,, ,, ..	40 ,, ,, ,,
,, 13, ,, ...	2·55 ,, ,, ..	55 ,, ,, ,,
,, 26, ,, ...	3·83 ,, ,, ..	60-65 ,, ,, ,,

Differential Count: *May* 5, 1921.—Polymorphs 50 per cent; lymphocytes 45 per cent; mononuclears 5 per cent (polychromatophilia marked).

The hæmoglobin was estimated, by comparison in a Gowers hæmoglobinometer with a control blood—as the standard tint was obviously unreliable.

Examination of urine: *April* 28, 1921.—Very dark brown, strongly alkaline, heavy cloud of albumin. No bile pigments; deposit numerous granular casts.

May 3, 1921.—Clear yellow, very faint trace of albumin; numerous granular casts—some apparently formed by ghosts of red cells.

May 18, 1921.—Alkaline yellow; no albumin; no casts.

My thanks are due to A.D.P. Mesopotamia, Lieutenant-Colonel Hamerton, D.S.O., R.A.M.C., for valuable advice during the progress of the case—and to Lieutenant-Colonel H. Herrick, C.M.G., D.S.O., R.A.M.C., for permission to publish these notes.

THE RELATIVE INCIDENCE OF INJURIES BY GUNSHOT WOUNDS TO PERIPHERAL NERVES.

By CECIL WORSTER DROUGHT, M.A., M.D.Cantab., M.R.C.P.Lond.

Physician to the Neurological Department of Bethlem Royal Hospital; Assistant Physician, West End Hospital for Nervous Diseases; late Captain Royal Army Medical Corps, and Consulting Neurologist, Woolwich Military District.

The following is a statistical survey of 1,602 cases of gunshot wounds of peripheral nerves, comprising 1,944 nerve lesions personally examined from 1914 to 1920. As the total number of cases is rather larger than most published individual records (e.g., 639 cases by Tinel and 520 by Purves Stewart and Evans) they may, perhaps, give a more correct idea of the proportion in which the different nerves are affected both separately and in association with other nerves. The lists are arranged in order that the frequency with which any individual nerve is injured may be compared with the number of times that nerve is injured in conjunction with other nerves—thus, the figure appearing after the word "alone" refers to the number of cases of isolated injury to that particular nerve, while the figure following the word "total" indicates the *total* number of cases of injury to the nerve in association with other nerves as well as alone. Multiple injuries are also classified separately from isolated lesions.

Owing to the difficulty of diagnosing a pure nerve injury in the case of the optic and auditory nerves, the first and eighth nerves are intentionally omitted from the list of cranial nerves. To illustrate the relative frequency with which different portions of the brachial plexus are involved, injuries to this important structure are considered as affecting (1) the roots of the nerves forming the plexus; (2) outer cord; (3) posterior cord; (4) inner cord; and (5) two or more cords.

Cranial nerves.

Motor oculi (III)	Total,	4	Alone,	4
Trigeminal and branches (V)	,,	13	,,	8
Abducant (VI)	,,	3	,,	2
Facial (VII)	,,	37	,,	27
Glossopharyngeal (IX)	,,	1	,,	—
Vagus and branches (X)	,,	4	,,	—
Spinal accessory (XI)	,,	12	,,	7
Hypoglossal (XII)	,,	9	,,	3

Cranial nerves (continued).			
Multiple :—			
V and VII			5
V, IX, X and XII			1
V and XI			1
V and cervical nerves			1
VII and XII			3
VII and cervical nerves			1
VII and brain injury			1
X and XII			1
XI and XII			1
XI, XII and cervical sympathetic			1
XI and cervical nerves			1
XI and brachial plexus (outer cord)			1
XII and cervical nerves			1
Total cranial nerve injuries, 85.		Total cases	70
Cervical sympathetic		Total, 12	Alone, 7
— with XI and XII nerves	1		
,, cervical nerves			1
,, brachial plexus			2
,, cervical portion of spinal cord			1
		Total cases	81
Cervical nerves (excluding roots of brachial plexus).			
Cervical plexus		Total, 19	Alone, 10
Multiple :—			
3rd and 4th cervical nerves and V nerve	1		
,, ,, ,, VII nerve	1		
,, ,, ,, XI nerve	1		
,, ,, ,, XII nerve	1		
,, ,, ,, cervical sympath.	1		
,, ,, ,, roots of brachial plexus			3
,, ,, ,, spinal cord lesions			1
		Total cases	95
Brachial plexus			
Roots of brachial plexus		Total, 35	Alone, 31
— with cervical sympathetic	1		
,, 3rd and 4th cervical nerves	3		
Outer cord		,, 24	,, 8
,, and XI nerve	1		
Posterior cord		,, 53	,, 34
Inner cord		,, 40	,, 18
,, and cervical sympathetic	1		
Outer and posterior cords			2
Outer and inner cords			4
Posterior and inner cords			8
Outer, posterior and inner cords			9
Total cases of brachial plexus injury, 120.		Total cases	209
Other nerves of the upper limb.			
Musculo-spiral		Total, 250	Alone, 199
Median		,, 323	,, 173
Ulnar		,, 330	,, 205
Musculo-cutaneous		,, 24	,, 1
Circumflex		,, 4	,, 4
Internal cutaneous		,, 49	,, 10
Lesser internal cutaneous		,, 2	,, —
Posterior interosseous		,, 22	,, 15
Radial		,, 9	,, —
Anterior interosseous		,, 1	,, 1
Multiple :—			
Musculo-spiral and median			26
,, and ulnar			12
,, and musculo-cutaneous			6
,, and internal-cutaneous			2

Other nerves of the upper limb (continued).

Multiple :—	
Median and ulnar	84
,, and musculo-cutaneous	6
,, and posterior interosseous	1
Ulnar and internal cutaneous	16
,, and posterior interosseous	2
Posterior interosseous and radial	1
Median and radial	4
Musculo-cutaneous (branch of) and radial	1
Internal cutaneous and lesser internal cutaneous	1
Musculo spiral, median and ulnar	2
Median, ulnar, and musculo cutaneous	2
,, ,, and internal cutaneous	12
,, posterior interosseous and radial	2
,, ulnar, and posterior interosseous	1
,, ,, and lesser internal cutaneous	1
Musculo-spiral, median, ulnar, and internal cutaneous	2
,, ,, ,, and musculo-cutaneous	1
Median, ulnar, musculo-cutaneous and internal cutaneous	6
Total cases	1,008

Nerves of trunk.

Suprascapular	Total,	2	Alone,	2
Long subscapular	,,	1	,,	1
,, thoracic	,,	1	,,	1
Intercostal	,,	10	,,	10
			Total cases	1,022

Cauda equina	24
Lumbo-sacral plexus	4
Ilio-hypogastric	2
Ilio-inguinal	3
,, and genito-crural	1
Total cases	1,056

Other nerves of the lower limb.

Sciatic (trunk)	Total,	189	Alone,	184
Small sciatic	,,	15	,,	10
External popliteal	,,	162	,,	126
Internal ,,	,,	76	,,	42
Musculo-cutaneous	,,	29	,,	26
Anterior tibial	,,	13	,,	10
Posterior ,,	,,	64	,,	56
Anterior crural	,,	14	,,	12
External cutaneous	,,	4	,,	3
Middle ,,	,,	6	,,	6
Internal ,,	,,	5	,,	4
,, saphenous	,,	12	,,	6
Obturator	,,	2	,,	1
External saphenous	,,	4	,,	2
Plantar	,,	2	,,	2
Multiple :—				
Sciatic and small sciatic				5
External popliteal and internal popliteal				34
,, ,, and external saphenous				2
Anterior crural and external cutaneous				1
,, ,, and obturator				1
Internal cutaneous and internal saphenous				1
Posterior tibial and internal saphenous				2
,, ,, and anterior tibial				2
,, ,, and musculo cutaneous				3
,, ,, anterior tibial and internal saphenous				3
Total number of nerves injured 1,944			Total cases	1,602

Reviews.

THE BIRTH AND EARLY DAYS OF OUR AMBULANCE TRAINS IN FRANCE, AUGUST, 1914, TO APRIL, 1915. By "Wagon-Lit." London: J. Bale, Sons and Danielsson, Ltd., 1921. Pp. 19. Price 9d. net.

This is a "brochure" in which the author has reviewed graphically and in an interesting way the ambulance train work in France from the outbreak of war.

He shows difficulties that existed, the ways in which these were overcome, and the results which were obtained by the loyal co-operation of every member of the staffs employed.

He pays a generous tribute to all those who strove so hard to bring help to the sick and wounded.

It is a story which should be read by everyone in the Corps.

To many it will bring back memories of hard and difficult times yet, as the story is told, they will look back with pride and pleasure and rejoice that their work has been so remembered.

The "brochure" deals in no way with organization or the administrative side of ambulance trains.

The author might with advantage to many follow this with a more technical account, and enable others to benefit by his particularly large experience.

WHAT TO DO IN CASES OF POISONING. By William Murrell, M.D., F.R.C.P. Twelfth edition. Revised by P. Hamill, M.D., D.Sc., F.R.C.P. London: H. K. Lewis and Co., Ltd. 1921. Pp. vi and 273. Price 4s. 6d.

This handy and popular little book has been thoroughly revised by Dr. Hamill but in such a way as to "preserve the characteristic style." Some of the paragraphs have been entirely rewritten, for example that on salvarsan, and, throughout, the question of treatment has received more attention.

In the preface the Reviser explains the omission of the various poison gases used in the late war, "dope" and "T.N.T." as unlikely to be met with in civil life and their inclusion would defeat the object of keeping the book as compact as possible.

A TEXT-BOOK OF MEDICAL JURISPRUDENCE AND TOXICOLOGY. By John Glaister, M.D., D.P.H.Camb., F.R.S.E. Fourth Edition. 137 illustrations and 1 coloured plate. Edinburgh: E. and S. Livingstone, 1921. Pp. xv and 902.

Professor Glaister's "Medical Jurisprudence" has now reached its fourth edition, and as the second and third editions appeared in 1910 and 1915 respectively it has the advantage of having been carefully revised from time to time and kept up to date.

In this edition the main additions deal with industrial and other poisonings, lunacy law and the relationship of intoxication to responsibility for crime.

The book is divided into two sections: I, Medical Jurisprudence; II, Toxicology.

The opening chapters discuss the scope of the subject and the medico-legal aspect of the Medical Act, the Venereal Diseases Act and the Workman's Compensation Act. Particular attention is given to medical evidence and to legal criminal procedure as, according to the author, the reason that medical men usually make poor witnesses is the lack of knowledge of such procedure coupled with want of familiarity with law courts.

The important and, at present, very vexed question of professional privilege and secrecy is discussed in an able manner and the points are illustrated by reference to and extracts from important cases.

Little need be said of the main subject matter which is excellent. The subject is comprehensively dealt with and punctuated with full descriptions of illustrative cases including well-known and important criminal cases. The chapter on blood-stains and their identification is good.

Pages 627 to 862 are devoted to toxicology and the important industrial poisons, which have come into prominence as the result of the war, such as T.N.T. and "dope," are adequately dealt with.

The book is excellent for both students and practitioners. Indeed, such a book is essential as a book of reference for a medical man who may at any time in the course of his practice be called upon to give evidence in a court of law.

J. C. K.

THE HEART. OLD AND NEW VIEWS. By H. L. Flint, M.D. London: H. K. Lewis and Co., Ltd., 1921. Pp. xii + 177. Price 15s. net.

Excellent alike in manner and material, this little book provides an interesting survey of cardiac lore from the earliest times to the present day.

From the mysticism of the ancients with their theories of the soul and the humours, through the Galenic period with its imperfect long-lived doctrine and the sterility of the dark ages, we are led on past the anatomical triumphs of Vesalius and the invention of the stethoscope by Laennec to the epoch-making discovery of Harvey. Thence in our own times we pass to the work of Mackenzie on the interpretation of the venous pulse and the classification of cardiac irregularities, and to that of Einthoven on the string galvanometer, the foundation of modern electrocardiography.

The modern views of cardiac disorders, and particularly the interpretation of venous and electrocardiograph tracings, are clearly set out, supplying in brief an excellent summary of the most recent work.

ORTHOPÆDIC SURGERY OF INJURIES. Vol. i and ii. London: H. Frowde, Hodder and Stoughton, 1921. Vol. i, pp. xv and 540; vol. ii, pp. viii and 692. Price £4 4s.

The surgery of most war injuries has now been pretty thoroughly discussed, though papers still continue to appear in American medical journals, but a complete history of the reconstructive work on limb injuries remained to be written till the present work appeared. It is the more welcome as being under the editorship of one of our best known orthopædic surgeons and as representing the views of his school. Sir Robert Jones contributes three articles and a preface in which the true object of orthopædic surgery is stated to be the restoration of function. "The orthopædic mind is trained to think in terms of function." The establishment of orthopædic centres during the war is described, the value of voluntary rather than passive movements is insisted upon, and the necessity for a sound knowledge of general surgery in orthopædic work is pointed out.

The first volume begins with an article by Sir Arthur Keith on the work of H. O. Thomas, an appropriate compliment to the founder of the Liverpool School of Orthopædics, and continues with an article by Sir Henry Gray on the value of co-operation in the medical services in the field for the prevention of deformities. Those who have heard Dr. Goldthwait's energetic exposition of his views at his Boston clinic will read with interest his chapter on the application of orthopædic principles to military needs.

The next five chapters are concerned with fractures, and the chronic osteomyelitis which results from septic fractures. Ununited fractures are in the

capable hands of Mr. Hey Groves, while the editor deals with malunion of the femur. An article on splinting by Captain H. G. Carlisle is well illustrated and does justice moreover to the work of Major M. Sinclair, C.M.G.

A consideration of the orthopædics of joint injury occupy the seven succeeding sections. Among the names of contributors, which include Messrs. Naughton Dunn, Daw, Alwyn Smith and Aitken, appear those of two American writers. It is not quite easy to appreciate the exact bearing of Mr. Kedner's paper, excellent as it is. There is very little mention of the hip joint in it. The section is wound up by two articles on stiff joints and flail joints by the editor. The last two sections of volume I deal with amputations (Mr. Elmslie) and the fitting and training of limbless patients by Sir John Lynn Thomas.

Volume II begins with the consideration of injuries to peripheral nerves with their anatomy (excellently illustrated), diagnosis and prognosis, and their operative and post-operative treatment. End results are dealt with by Mr. Alexander and Miss Forrester-Brown. This is a very valuable part of the work and should be attentively studied by all who have the care of these difficult and often disappointing cases. Tendon transplantation, that much debated subject, is discussed by Mr. M. C. Murray, and then follow two sections on injuries of the head and spine by Dr. Farquhar Buzzard and Mr. Percy Sargent. Admirable in themselves as these sections are, they do not seem to have an obvious bearing on the subject. Some curious voluntary movements noticed in limbs, the nerves and muscles of which are paralysed, are explained by Professor Wood Jones.

The final chapters deal with splints, electro-therapy and massage, exercises, hydrotherapy and skiagraphy, and in the last chapter Dr. Hill gives his experiences of the organization and administration of a military orthopædic hospital.

It will be seen that the main object of the book is to describe orthopædic methods as applied to war wounds, but it also has an important bearing upon civil and industrial accidents. With a multiplicity of authors some inequality of treatment is to be expected, but on the whole the book is worthy of the subject and of its editor.

Correspondence.

TREATMENT OF AMŒBIC DYSENTERY WITH EMETINE BISMUTHOUS IODIDE.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—In reading the "Notes on Treatment with Emetine at the University War Hospital, Southampton," in the June number of the JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, I was interested, but not surprised, to find that treatment of cases of *Entamœba histolytica* with salol-coated pills of emetine bismuthous iodide proved unreliable.

I had the misfortune to become infected with *E. histolytica* and recently was subjected to a course of injections of emetine hydrochloride followed by tabloids of emetine bismuthous iodide by the mouth.

Much to my surprise and pleasure, I found the tabloids caused me no nausea or discomfort of any kind. A few days after I began taking emetine bismuthous iodide by the mouth a complete tabloid was discovered to have passed, apparently quite unaffected, through the alimentary tract. Doubtless many salol-coated

pills of emetine bismuthous iodide share the same fate and therein lies a possible explanation of the advantage of the emetine bismuthous iodide mixture referred to in the notes above mentioned.

British Station Hospital,
Rawal Pindi.
July 17, 1921.

I am, etc.,
F. S. IRVINE.
Lieutenant-Colonel, R.A.M.C

MUMPS.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—The following experience in mumps may prove interesting to your readers. Towards the end of 1917, I was placed in charge of a malaria convalescent camp, in the Murree hills, for Indian troops returning from East Africa. It was early winter and after the first week in December the camp was under three feet of snow. There were two units, in all about 800 strong, made up of Hindu and Mohammedan companies as usual, approximately in equal proportion.

Within a week of their arrival, on October 15, the first case of mumps appeared in a Mohammedan sepoy. On inquiry it was found that the disease had been going on in one unit, for several months, and had been so common, that hardly any notice was taken of fresh cases. Within a further ten or fifteen days, at least thirty more cases occurred, all among Mohammedans. This incidence was so interesting, that I immediately set about investigating the cause of the spread. As is well known the saliva is the principal if not the only vehicle of the infection.

It must be remembered that the Hindus always eat by themselves, while the Mohammedans always eat in small messes, of five or seven. There is a saying that if there be seven in a family of Hindus, there is sure to be eight kitchens, meaning that the Hindu not only eats by himself but cooks by himself as well. The sepoy sticks to his custom, which is essentially of a religious nature, quite as much as the opposite Mohammedan gregarious habit. In both the food is very similar, and of a very primitive nature, eaten without implements, save the hands, out of a wooden or earthenware receptacle. The food is always of a semi-fluid pultaceous nature. It is a fact that not a single Hindu got infected. And a consideration of the habits already explained leaves no doubt on the way the infection was spread from man to man.

While on the subject of mumps, I may be allowed to mention a point or two as to diagnosis. While it is generally admitted that this is easy, I have recently seen a case in an adult of 19, which shows a possible source of error. He was a young recruit, and reported sick with a large parotid swelling, which was seen by two surgeons. They both thought of mumps, and I saw the case later, in due course. Within eight days the diagnosis was clear. There was no fever all along, and the swelling remained one-sided. As soon as the patient was able to open his mouth and the teeth were examined, it was observed that the lower wisdom tooth on both sides was unerupted. An impacted molar was diagnosed, and the skiagram confirmed the diagnosis. Shortly afterwards a patient came to hospital, suffering from a double pre-auricular swelling, extending well round the ear on both sides, so as to appear on superficial examination, very much like double

mumps. Added to this there was high fever, and other constitutional symptoms. The orderly medical officer who admitted him, considered it a case for isolation. On examining the man, shortly after admission, I was struck by the symmetry of the swellings, and the remains of a boil on the middle of the forehead. On closer examination there remained no doubt that it was a case of septic absorption from the base of the apparently healed boil.

I am, etc.,
J. E. H. GATT.

EYESIGHT, SPECTACLES AND RIFLE SHOOTING.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—I have read with interest the articles by Captain William Wallace, M.D., "The Vision of the Soldier," and would like to add a few comments with special reference to his remarks on p. 124 of the August number of the Journal in which he deals with spectacles and musketry.

I have, for a considerable number of years, been interested in the question of the use of spectacles in connexion with rifle shooting, and can write with some authority, also with the hope that my experience may prove of interest to your readers.

I am in entire agreement with Captain Wallace when he says that to obtain the best result by the aid of glasses one must use decentred lenses (i.e., those in which the optical centres are displaced from the geometrical centres). I had such glasses constructed for use at Bisley in 1920, with gratifying results.

To obtain the maximum benefit, the Toric lens ought to be used, and this should be fitted to a *hinged* frame, the object being to adjust the plane of the glass in such a way as to make it at right angles to the desired position of visual alignment. Inasmuch as the prone position is the one almost invariably adopted for target shooting, the advantage of the hinge is obvious.

As to the degree of decentring, this depends on the reciprocal position of firer and rifle. Some lie almost, or quite straight to the rifle; others at a very acute angle; some very close to the ground, others rest high on the elbows.

Consequently, in fitting glasses for rifle and target shooting, it is necessary to decide the point of displacement of the centring, by actual experiment for each individual firer, in accordance with the personal equation.

For this purpose it is required that the optician lie immediately opposite the firer (who has his rifle in position).

He (the optician) should then carefully watch, and mark the exact place on the glass through which the firer aims, in order to determine the precise decentring point.

I find that ninety-nine per cent of rifle-shots who use glasses for optical purposes do not employ decentred lenses, and I am convinced that if the advantage of employing them was better known, much improvement in rifle shooting would follow in those who have to use spectacles.

As regards Bisley, a very large number of competitors only use glasses to protect their eyes from the sun. The glasses mostly worn are of the green, or greenish yellow colour, of various degrees of shade. I personally use two pairs,

an ordinary white clear pair, and a light tint of green and yellow called chlorophil. It has been found that this greenish or greenish-yellow colour cuts off all the actinic rays and helps to give a clean-cut defined edge to the aiming point, when shooting at targets in which the aiming point is irregular and ill defined, the colour of the target being made up of neutral tints as in the standard musketry figure target of 1920; they have less value in the "tin-hat" target of 1921, and still less in the bull's eye target (black bull's eye on a white ground) except that they diminish glare.

In the case of high or even moderate myopes in prescribing glasses for shooting, it is impossible to get a lens that will, at the same time, give clear definition of fore-sight and target, even if a peep back-sight is used instead of the Government pattern U.

The question is, what is the best to prescribe? and without hesitation I say, use a lens which will clearly define the fore-sight, even if it leaves the target in a slight haze.

The winner of the Grand Aggregate, and of the Service Rifle Championship at Bisley in 1914, used this kind of lens in his shooting spectacles. You cannot shoot with a hazy or blurred fore-sight, especially in changing lights.

To revert to the question of decentring the optical centre: at Bisley there are two distinct kinds of rifles used, the Service one, fitted or otherwise with a peep-sight, and which is used in ninety per cent of competitions, and all are in the prone position, and the match rifle proper.

The match, or any rifle is fitted with optical sights, and a spirit level, these give a magnification of between plus three and plus four. The back-sight of this rifle is fitted at the very end of the butt. To shoot with this rifle one has to use the back position with feet towards the target. Sighting through the optical centre (i.e., the midpoint of the principal axis) is the best way if using glasses for this type of rifle shooting.

The question arises, from a musketry point of view, is it worth while in our small army to accept anybody where the sight of the right eye is below normal, owing to the time, difficulty and expense involved in fitting those whose eyes are below normal with glasses suitable for musketry purposes?

As regards peep-sights or aperture sights for military rifles, Bisley has been using them for twelve years or more past.

A rifle was designed by the Government just before the war, which was fitted with a peep-sight, but war intervened, and this rifle had to go. We now hear from the Small Arms school at Hythe, that the next new rifle to be issued to the troops when money is available (when??), is to be fitted with a peep-sight, and that they now accept the peep-sight as the best type of sight for a military rifle. Why cannot they fit one to the present S.M. Lee Enfield rifle—the cost should not be prohibitive?

This decision is a move in the right direction. It will lessen strain on the eye in the first instance, as with a peep-sight, one has not to try and focus three points at the same time, viz., back-sight, fore-sight, and target, which, in those with anything other than normal vision often ends in either losing the fore-sight or the target.

One cannot lose the fore-sight if one has to look through the back-sight, two

points of focus instead of three. Here the fore-sight is as large as life, and has only to be put on to the object aimed at.

For all distances the peep-sight is the best.

Peep-sights mean three very important items in musketry: (*a*) accuracy; (*b*) rapidity of aim and fire; (*c*) clearness of definition.

It is much easier to teach shooting with an aperture or peep-sight, than with the old fashioned opened back-sight and fore-sight.

With the peep-sight there is no question of how much or how little of the fore-sight you are taking in, and no eye strain.

The fore-sight has to be placed in the centre of the peep or aperture sight, and the ordinary eye can centre the peep-sight automatically to $\frac{1}{1000}$ of an inch, if the peep is not too large say in and about 0·1 of an inch, an error of $\frac{1}{150}$ of an inch only means approximately 1 inch error on the target for every 100 yards you are away, provided the hold and let off is good.

The aperture-sight makes for definition as it cuts off circumferential rays.

The question is what sort of peep-sight will be adopted.

There are certain factors necessary in relation to this:—

(1) The peep-sight must be sufficiently strong and stable to withstand rough usage.

(2) The aperture should be bored in a disk or plate sufficiently large to prevent the "bottom of the aperture dropping out." If the circle of metal round the aperture is not sufficiently thick or broad this is quite likely to happen, if the aim happens to be prolonged, and in certain lights.

(3) The aperture itself must be sufficiently large to afford a good field of vision, and to give luminosity. A too small aperture is a great fallacy. I personally use 0·06, 0·07, 0·08 of an inch. 0·1 of an inch is probably better than any of these if one talks of musketry in contra-distinction to rifle shooting. It is a great mistake to think that a small aperture means accuracy; it does not—it means loss of light, which means inaccuracy. I have already said that the ordinary eye will centre to $\frac{1}{1000}$ part of an inch in a 0·1 inch aperture, in fact it would do so in an aperture a good deal bigger than 0·1 of an inch.

(4) The aperture should be placed sufficiently near the eye; probably the best distance is about four to five inches away when lying prone. The Americans make wonderfully fine shooting with their Springfield rifle which has been provided with an aperture for years (as well as an "open sight"), and their aperture is placed a good deal further away than this.

Probably the best place for the peep- or aperture-sight on a military rifle is immediately over the bolt-race about the position of the "charger guide."

In deciding the exact position it should be borne in mind that the longer the sight radius (i.e., the distance between the fore-sight and the back- or aperture-sight) the greater the accuracy, as by lengthening the sight radius, mistakes in aiming are minimized on the target, and it is for this reason that in the match rifle which is shot from the "back position" the fore-sight is on the muzzle of the rifle and the back-sight is at the extreme end of the butt.

To my thinking Army Medical Officers should have a very clear insight into the relationship of eyesight and musketry, and the kind of spectacles required for the type and condition of musketry practised in the Army for the time being.

In conclusion I would say that when testing men for spectacles for musketry, or for rifle shooting as practised at Bisley in contra-distinction to musketry, to get the best results the person tested must be absolutely true to the person testing, whether he be the ordinary medical officer or the ophthalmic specialist. If the man being tested is not out to help no good results can be expected, no matter how expert or cunning the tester may be. And as my last word I would re-echo Captain Wallace—You cannot expect to train a man who has never worn spectacles into a marksman the first time he wears spectacles. The eye must in the first instance be educated to the lenses used, as a certain time must elapse before full advantage will be experienced. A man should therefore wear them always when at aiming drill and snapping practice, and should not bring them out of his pocket, merely when he lays down to fire. Better still, they should be worn at all times in order to educate the eye to their use, or I should rather say, re-educate the eye to the lens used. There are some men, however, irrespective of their eyesight be it good, bad or indifferent, be they fitted with glasses or not, who can never be turned into even third class shots; this is due to want of intelligence and inability to make deductions, and to weigh up factors from shots already fired.

I am, etc.,
LANGFORD LLOYD,
Lieutenant-Colonel, Royal Army Medical Corps,
Member of many International Teams, and Member of Council National Rifle Association.

IMPORT DUTIES IN INDIA.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

DEAR SIR,—Officers coming out here next trooping season might like to know that the new import duties are much more serious than they used to be; they were enhanced on March 1 last as follows:—

Motor cars and motor cycles subject to twenty per cent ad valorem.

Guns and rifles twenty per cent with a minimum of fifteen rupees, which means that on a ·22 rifle costing 35s. in London one pays fifteen rupees, and is therefore hardly worth importing.

Cartridges are also subject to the twenty per cent duty.

As so many officers wish to bring motor cycles out nowadays—and, indeed, in some stations the work cannot be done without them—they may wonder if it is worth while bringing them.

I would strongly advise anyone who intends using one, to bring it out, as owing to the depreciation of the rupee prices have risen considerably out here, no importation is going on by dealers owing to the uncertainty as to which way the rupee is going—therefore the choice here is very limited. If a motor cycle is bought and used, its depreciation in value as a second-hand article can be deducted in assessing the value for customs purposes.

Petrol can be obtained on payment from the S. and T. at rupees 1.8 per gallon. Lubricating oil costs rupees 7.8 per gallon.

Under recent regulations officers are entitled to draw syce and forage allowance in lieu of keeping a horse if by so doing their duties can be more expeditiously performed, which is often the case.

There are other tips which I cannot go into in this short space, but I should be pleased to answer any inquiries and give my advice for what it is worth to anyone coming out. I know how difficult it was to get information on the changed conditions on coming out for a second tour, and received an unpleasant surprise when I was called upon to pay about 500 rupees customs duty before I could land my baggage.

A field officer can only draw 500 rupees advance on landing, and I think captains 300 rupees. It is sound to take your month's advance of pay before leaving England whether you want it or not for various reasons.

The Fort, Allahabad, India. I am, etc.,

July 27, 1921. F. B. LATHBURY,

Major Royal Army Medical Corps.

N.B.—A married officer taking his full allowance of entitled baggage on board will loose heavily on proceeding up country out here, as the entitled amount is much lower here. A major is allowed fifteen maunds free on his warrant (maund = eighty pounds), the rest must be paid for by the individual. No travelling allowance is admissible.

VACCINATION TREATED LOCALLY WITH CASTOR OIL.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—I have found the following procedure very useful.

Take some cotton wool and dab over the whole inflamed area with castor oil. Take a piece (square) of boric lint and cover it up, pinning the lint in position. Put the arm in a sling, excuse the case all duties, and direct him to attend daily for the same treatment.

The lint should be large enough to enfold the arm loosely, the opposite (diagonally) corners to be fastened inside the arm with a safety pin. The upper corner to be similarly fastened to the inside of the shirt or vest, but near the shoulder. The lower corner lies loose.

This is not a lightning cure, but from the very first application all untoward symptoms are checked, the inflammation begins to subside and continues to do so.

I am, etc.,

Chichester. E. H. MYLES,

September 19, 1921. *Major, Royal Army Medical Corps.*

PHYSICAL TRAINING IN THE ARMY.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—I have read with considerable interest the letters on the above subject from Lieutenant-Colonel Cotton and Major Lothian in recent issues of the Jôurnal, further ventilation being invited.

First, as regards the initial training at the depot. It is excellent as far as it goes but much too short—lasting about seven weeks when the recruit—only half-taught, is hustled off to join the service unit.[1] As far as my memory goes, at the Guards Depot, Caterham, three months is the regulation and this ought to be made applicable all round, besides it is well to bear in mind that the depot stands for the recruit in the same relation as the preparatory school for the public schoolboy.

Whilst on this subject I would like to bear testimony to the admirable manner in which these establishments are organized as compared with old days, and I can claim to speak with a certain amount of authority, as during six years (1908-14) spent at the War Office I practically inspected at every depot (all arms) in the United Kingdom, whilst as a junior officer I was in medical charge of two different Brigade Depots—as they were then styled—in the early eighties.

My usual practice when visiting these places was to go straight to the gymnasium where the recruits (regulars and special reserve) were drawn up separately: the gymnastic instructors were, as a rule, very keen about their work, gauged each man's capacity, kept accurate records and it was remarkable how the recruits in a short time improved in physique—especially as regards *weight*, generally a reliable test—some of them gaining fourteen pounds and over. I may add that the rations were always pronounced excellent and on this subject I never at any place heard a complaint. This satisfactory state of affairs I attribute to two innovations made by the Army Council when they assumed control of the Army. The first was vesting the command of the depots in Majors still on the active list: these officers I invariably found took the keenest interest in their work and in the welfare of the recruits under their charge, but without exception they regarded the period of training as far too short. I often represented this to the Adjutant-General on my return to Headquarters, and he fully concurred, but the demand for men for the service battalions, etc., was so urgent that no extension could be given.

The second factor was the appointment—at the instance of my predecessor—of Medical Inspectors of Recruits: these officers have fully justified their existence for many reasons that I need not now enter into. Of course it will be understood that the above remarks apply to pre-war days, but I have no reason to imagine that there has been any change in the wrong direction since that time.

However, the crucial point now to be decided is whether the physical training (three months let us hope) which a man has undergone as a recruit is to be maintained when he has joined the service unit. There is much to be said in its favour but I think this is a matter on which the opinion of commanding officers ought to be obtained before any action is taken; the one objection that I can

[1] Since the foregoing was written I have been informed that the time spent by recruits at *all* depots has been considerably increased.

imagine is that, when a man has undergone a strenuous course at the depot, he is apt to be "fed up," to use a colloquialism, if he has still to continue it as a trained soldier.

As an alternative, I can only suggest increased attention to what is after all physical training in an attractive form, viz., athletic sports, hockey, football, boxing and, best of all, cross country running by teams, neither ought route marching to be lost sight of: one of the items in the "Kitchener test" instituted in India in 1904 was a fifteen miles march under service conditions—ten marks being deducted for each man falling out, and it was astonishing how very few in a battalion between 800 and 900 strong *did* fall out, which spoke well for the men's condition.

As regards the reservists, I doubt very much if—as Colonel Cotton suggests—any considerable proportion would continue the daily exercises in civil life. I noticed this class particularly in my own division during the South African war and although, on rejoining, many were of the portly and flabby variety, six weeks on the veldt soon got them into form and in the long run they were as good as anyone could wish.

In conclusion, I would like to mention one type of recruit that always impressed me in old days. When Principal Medical Officer some twenty years ago, of the Home District, as it was then called, and which—in addition to Caterham—included other stations such as Guildford, Reading, Oxford, etc., I was much struck by the excellence of the London carmen (e.g., Carter Paterson and Co., etc.): these men from the nature of their calling—always handling heavy packages and living an open air life in all weathers—were of superior physique and in addition were *remarkably* intelligent. The commanding officers of depots had a high opinion of them and I imagine that a battalion composed exclusively, if that were possible, of London carmen would be hard to beat.

I am, etc.

LAUNCELOTTE GUBBINS,

Lieutenant-General (late Director-General Army Medical Service.)

Wimbledon,
September 29, 1921.

ARMY MEDICAL SERVICE.

ROYAL ARMY MEDICAL CORPS.

To be Colonels Commandant: Lieut.-General Sir Alfred Keogh, G.C.B., G.C.V.O., C.H. (honorary Colonel R.A.M.C., T.F.), retired pay; Lieut.-General Sir A. T. Sloggett, K.C.B., K.C.M.G., K.C.V.O., retired pay; Major-General Sir T. J. Gallwey, K.C.M.G., C.B., retired pay; Surgeon-General (ranking as Major-General) Sir F. W. Trevor, K.C.S.I., C.B., retired pay.

As is in the case of the selection of Colonels Commandant of other Corps, two officers were selected for distinguished services during the recent war and two according to seniority on the list of Generals. A rule has been made that no officer should be appointed who is over 70 years of age and a selected officer relinquishes his appointment on attaining that age.

Notices.

EDITORIAL NOTICES.

The Editor will be glad to receive original communications upon professional subjects, travel, and personal experiences, etc. He will also be glad to receive items of news and information regarding matters of interest to the Corps from the various garrisons, districts, and commands at home and abroad.

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Original Communications.

SOME FACTORS IN BONE-REPAIR.

By WILLIAM SEAMAN BAINBRIDGE.

Commander Medical Corps, United States Navy, Reserve Force, New York City.

WHEN confronted with a fracture of one of the long bones, the surgeon, like the anxious miner straining his vision for the first glitter of gold in the ground, is inclined often to focus his entire attention upon the local lesion, with a zeal which would be highly commendable were it not for the fact that this fixation of his mind upon a simple point is apt to result in the disregard of conditions operative at a distance from the broken bone, but nevertheless of important bearing upon its repair. His curative endeavours are centred at once upon the mechanical adjustment of the fragments, and his aim is their direct longitudinal apposition, without lateral deviation, after conscientious removal of any infection from without and equally careful exclusion of all foreign material, which here means also muscular or other tissue intervening between the separated bone-ends. The fracture having been "set" to his satisfaction, the expert surgeon proceeds to utilize the best appliances at his command for holding the fragments in place, without undue pressure or laxity, so that the circulation may not be impeded by an overtight appliance nor harmful excursion of the bone permitted by a loosely fitting one.

Among mechanical appliances used in the management of fractures of the long bones, the Thomas splint is generally credited with being a most useful and practical splint for the thigh and leg. The half-ring modification, or the Hodgen wire cradle extension suspension splint, are accorded the preference in certain cases. In the experience of many war-surgeons the Lane metal bone-plate was found to be excellently adapted to the reduction and immobilization of various types of fracture. The soft

crucible and tool steel used in the manufacture of the Lane bone-plates and screws is advantageously replaced in Sherman's modification, by stronger plates and screws constructed of elastic vanadium steel. Some valuable by-products of the unprecedented clinical experience of the world-war, applicable also to the requirements of civil practice, are represented by Willem's screw extension for fractures of the thigh and leg bones; Blake's extension apparatus for the treatment of complicated femoral fractures; Sinclair's stirrup and splints; the practical ice-tongs calipers devised by Besley and modified by Pearson; the narrow flexible cast-iron bands of Sir Robert Jones; Magnuson's absorbable ivory plates and screws; the animal bone plates and screws recommended by Brougham and Ecke, Brenizer's intramedullary and extra-cortical beef bone splints for the repair of fractures of the long bones, Henderson's beef bone screws for fractures and bone transplantation. The admirable output of human ingenuity and professional skill embodied in these and other modern methods of fracture treatment has undoubtedly resulted in the saving of many limbs, without discrediting some of the older tried and tested appliances, such as Lambotte's extensible steel fixateur or skewer, and Reclus' walking stirrup for the treatment of delayed consolidation of leg fractures. During the reconstruction work at United States General Hospital No. 29, Fort Snelling, Minnesota, some new improved splints and appliances have very recently been developed, including an attachment for the Thomas leg splint which may be used with either a full length or shortened splint, and on each side. The method of fastening the attachment to the Thomas splint permits of adjustment to fit the length of any thigh. The advantages of this method are that by its use fractures of the femur can be treated with the knee flexed and active, or passive motion at the knee may be employed.

In the treatment of fractures which refuse to unite, bone-grafting is often successfully resorted to, especially in the form of Albee's inlay cortical graft. Intramedullary pegs have given satisfaction in fractures of the radius or ulna, whereas long, stout, inlay grafts give the best results in the case of the humerus and femur. The fact that in old un-united fractures, the bone near the site of fracture is sclerosed and avascular, interfering with the establishment of a sufficient blood supply, is unfavourable to the outcome of a grafting operation and at the same time renders the employment of very prolonged fixation inadvisable.

All these modern methods of fracture-treatment, the outgrowth of incomparable experience in handling large series of cases in military practice, are important, and some have a wide sphere of usefulness, but it must be kept in mind that the union of fractures also depends upon a number of factors entirely outside the adjustment of the loose bone-ends and their maintenance in place. Without exception, the fixation methods, including those which permit a certain amount of motion between the fragments and thus provide for better nutrition, unfortunately expose to

the danger of delayed union, sometimes with bending and buckling of bone and extensive callus-formation. There is no method of fixation at our disposal which directly favours or hastens bone-union and repair of fractures. This is a very important truth, especially in view of the advanced status of orthopædic technique which is hardly capable of further improvement, for we are thus obliged to look for improved results of fracture treatment in an entirely new direction. As admirably stated by Sir Anthony Bowlby, all young surgeons must be trained to realize that the so-called "setting" of the fracture should be really only the beginning and not the end of surgical treatment.

There are some factors which undoubtedly militate against a favourable course of bone-repair, but which from a strictly therapeutic viewpoint have been almost disregarded in this connexion. When called upon to initiate or supervise a process of repair in a fractured bone, the surgeon should keep in mind certain conditions under which bony growth is more or less interfered with. In the first place, *endogenous infectious processes* in the bone-marrow may lead to local congestion and disintegration. In the explanation of rickets, some investigators have assumed the action of known and unknown infectious substances which are carried, especially to the points of physiological hyperæmia of growing bones, at the ossification centres. Epidemics of rachitis and osteomalacia, caused by a diplostreptococcus, were observed by Morpurgo (1907) in rats, and inoculation of this coccus reproduced the same bone-symptoms in other animals. It has also been shown that after infectious diseases such as measles, scarlet fever, and diphtheria, bacteria are deposited by the blood-stream in the vicinity of the epiphysis, where they may give rise to changes corresponding both macroscopically and microscopically to those seen in rickets. It is a rational conclusion that injurious factors of this kind which exert a weakening influence upon unbroken bones will retard or prevent the repair and consolidation of fractures. Curvature of the femur is known to occur occasionally in connexion with suppurative processes of the knee-joint, and is presumably due in part to bone-infection by a contaminated blood supply, besides the weight brought to bear upon the femur when the patient attempts to walk, to which it is commonly attributed.

The fact must not be overlooked that lesions of infectious origin, both from without and within, are of frequent occurrence in bone. External infections may extend inwards through the cortex of the bone, an internal infection travelling by the opposite route. The tendency of bone-infection is to extend widely beneath the periosteum, or through the marrow spaces, causing more or less extensive necrosis of the bone. Aside from the *Staphylococcus aureus*, which is the most common pathogenic agent in bone-infection of endogenous origin, other germs may be responsible, such as the *Streptococcus pyogenes*, the pneumococcus, the gonococcus, or the typhoid bacillus. In certain cases of acute inflammatory atrophy of bone following upon secondary infection of adjacent joints, the presence of the

gonococcus has been demonstrated. It might be worth while to investigate the relative frequency of gonorrhœa in fracture cases where healing is delayed. "La goutte militaire" must be regarded at least as a suspect in those cases in which no other explanation of the deficient callus formation and bone-repair is forthcoming. In view of the long persistence of the typhoid bacillus in the body, as shown by the frequency of healthy "carriers," who have recovered from the disease but still harbour the parasite, it is perhaps not an exaggerated precaution to ascertain the existence of a positive or negative typhoid history in all cases of delayed union, if not in all fractures of the long bones.

As regards *focal infection* and its rôle as a factor in the repair of bone, it is suggestive that according to recent investigations of Pemberton (1920), on the nature of arthritis and rheumatoid conditions, the army furnished 400 cases of chronic arthritis, 298 of which showed demonstrable surgical foci, mostly in the tonsils (208), the remainder in the teeth or in the genito-urinary tract. Removal of the infectious focus, and correction of the altered metabolism by regulation of the diet, were found to have a beneficial effect on these disturbances.

Constitutional diseases, such as tuberculosis and syphilis, are undoubtedly responsible for delayed union in a certain number of cases. It is noteworthy in this connexion that syphilis, according to recent investigations appears to have such a pronounced structural effect upon the metabolism of bone-tissue as to leave more convincing proof of an old luetic infection on the X-ray plate than is afforded by the Wassermann or Noguchi tests.

Other latent infections or uncorrected irregularities which have not yet been challenged as factors capable of affecting the growth and solidification of the callus are evidently responsible in a number of obscure cases of ununited fracture. A very instructive case which illustrates the leading part played by remote conditions in certain cases of delayed union, came under my personal observation in the United States Naval Hospital, New York, N.Y. Amputation to the thigh had been performed on one side, and the other femur presented a fracture, the fragments overriding four inches, a condition which had persisted until the tendons were shortened and the bone-ends could be placed in apposition only by forcible extension. Two Lane plates were adjusted and held the femur in mechanical apposition, performing a useful function by lengthening the contracted muscles, but at the end of some months no callus had formed, while the flesh wound had healed and the general condition was fair. There evidently was a flaw in the metal of one of the bone plates, for it broke—an accident otherwise difficult of explanation—under careful employment of physiotherapeutic measures or perhaps lifting the patient cautiously from chair to bed. It was hoped for a time that the resulting irritation through the broken plate, with the associated mobility of the thigh, would stimulate the production of callus, but the fracture refused to unite. After the plate

had been removed, it was possible to insert a bone-graft from the tibia and to hold the fragments in place without undue pressure by retaining apparatus. Still, practically no callus developed, although the soft parts healed readily, and the fragments were in perfect apposition. About this time the aggravation of certain symptoms which had existed for several months began to divert our attention which had naturally centred in the intractable fracture to the patient's general and particularly abdominal condition. Periodical attacks of vomiting, after nearly every meal, had been noted for nearly three months, and the clinical evidence in the form of general abdominal discomfort, obstinate constipation, and typical attacks of pseudo-cholecystitis strongly pointed to intestinal stasis. Abdominal section was performed, showing well-marked bands and adhesions, which were corrected, to the great relief of the patient, who, after the abdomen had been put in proper shape, vomited only three times as a direct result of the anæsthetic, after which all vomiting was definitely at an end. But the most interesting result of the abdominal intervention, from the viewpoint of the present consideration, was the post-operative production of a strong progressively solidifying callus in the fractured femur. The lesson taught by this experience is so convincing that all comments would seem to be superfluous, and I will only repeat that the healing of a broken bone imposes a tax upon the resources of the general organism for which the body must be fitted by removing or correcting irregularities of all kinds which may and do interfere with the normal course of bone-repair.

The gastro-intestinal tract, with its many side pouches, recesses, cul-de-sacs and extensions, such as the gall-bladder and appendix, for example, naturally represents an important centre of dangerous foci of infection. The existence of local disease will inevitably delay the repair of fractures, not only on account of the ever-present risk of metastatic localization of microbes or toxins at a point of lowered resistance, such as is offered by a fracture, but because of the associated lowering of the immunity balance due to the existence of infection and the more or less successful establishment of tolerance on the part of the organism. Being already taxed and perhaps exhausted by the fight against one enemy, the defensive mechanisms are sometimes unable to achieve a victory at the other front.

It is gradually being realized that in disablements of the human machine from any cause, the patient must be treated as a whole, instead of being considered for the time being as an inevitable but superfluous appendage to his goitre, malignant tumour or other diseased condition. This truth has long been appreciated by advanced clinicians, who would not dream of treating goitre, gall-bladder or gastric disease, for example, without giving the closest attention to the manner in which other presumably intact organs discharge their obligations. Among operating surgeons, however, there is perhaps still a tendency to concentrate all efforts on the part that is in immediate need of repair, and a corresponding inclination to wonder that an apparently highly successful operation sometimes fails

to accomplish the confidently anticipated result. A broken skull or thigh often absorbs all the attention of the surgeon, who lavishes his best skill and technical ingenuity on the repair of the existing lesion. But the ideal treatment of fractures must take in *more* than the broken bone and the resulting displacement.

So much excellent work was done in the direct treatment of fractures of the long bones in particular, in the unprecedented clinics provided by the World-War, that there is practically nothing left for me to add to this important phase of the subject. On the other hand, the reasons for delayed or missed union and for remote imperfect results in spite of the most up-to-date operative procedures, still remain to be collected, compared, and explained, so that it may not be unprofitable to pass in review some of the obstacles to bone-repair and restoration of function, with suggestions as to their origin and possible control.

The greatest stumbling-block in the way of tissue-repair is well known to consist of *infection*, and the lessons taught by the World-War are still fresh in our minds. The mere word suggests contamination of soil, purification of wounds, debridement, primary excision and suture. But one of the points I wish to emphasize is that infection does not necessarily come from the outside. As a matter of fact many injurious micro-organisms lurk within the human system, where under ordinary conditions they may do no apparent harm, but as a result of auto-infection due to exacerbation of a latent or quiescent microbism are in all probability capable of retarding or preventing the recuperative and reparative processes of the body. Recent war experience has drawn attention to this possibility which occasionally was found to account for the peculiar and otherwise inexplicable behaviour of apparently sterile wounds. The explanation is probably to be sought in the disturbance of the immunity balance—which every human body in the course of its existence has established for itself—by the abrupt onset of a more or less complete and persistent loss of local resistance at some point of the intricate human machinery. However, the condition still remains relatively unknown, although as regards their number and range, the disturbances produced by these internal foes are probably not inferior to diseases caused by microbes introduced from the external world.

Medical antisepsis must be constantly on the alert, not only against the enemies at the gate, namely at the site of the fracture, but also against those which do their nefarious work hidden behind the bulwarks of the body. Some of their most important and dangerous strongholds are represented by the *mouth cavity*, the *tonsils*, and the *nasal accessory sinuses*. All these structures not only harbour highly pathogenic germs but furnish a favourable centre for their distribution in the body at large. A certain degree of tolerance becomes gradually established through the constant presence of these agents, but the defence is apt to break down as soon as the infliction of an injury lowers the normal resistance of a given portion of the body.

With special reference to the relation between lowered resistance on the part of bones or joints and hæmatogenous infections, one of the earliest experiments along these lines (Schüller, 1880) showed that the introduction of tuberculous material by way of the lungs, followed on the same day by artificial contusion of the knee-joint or infliction of fractures, in rabbits and dogs, was regularly followed within two to four weeks by a progressive inflammation of the traumatized joint, with externally demonstrable articular swelling, found to be of tuberculous origin at the autopsy, some weeks or months later. The damaged bones were sometimes found to contain sharply outlined collections of tubercle bacilli. This experiment affords a good illustration of the rôle played by traumatism and lowered resistance in the localization of infectious material contained elsewhere in the body. Another suggestive case in the older literature (König, 1884) concerned a young man who had a kyphosis, with symptoms of incipient pulmonary tuberculosis, but was in fairly good condition when he sustained a fracture of the ankle. In spite of suitable treatment the foot remained swollen and abscess formation with fistula supervened. Unmistakable tuberculosis of the ankle-joint developed, and about six months later the patient died. At the autopsy, a small tuberculous bone-focus was found at the site of the fracture, with advanced synovial tuberculosis in the joint. This case was undoubtedly one of metastatic tuberculosis lodging in a non-resistant fractured bone. Osteomyelitis experimentation points in the same direction. Kocher showed that after intravenous injection of the pathogenic cocci, osteomyelitis developed only in those bone-areas which had previously been damaged by traumatism. Of recent years Murphy, of Chicago, and others have repeatedly emphasized the bucco-dental or tonsillar ætiology of joint suppurations and related disturbances. The accessory nasal sinuses are responsible in a certain number of the cases. If, under ordinary conditions of weak health, a normal joint is unable to resist the invasion of bucco-pharyngeal microbes, but reacts in the form of pain and disablement, it is not surprising that in the presence of a local focus of infection the repair process of a broken bone should be delayed or very imperfectly accomplished. Even in the absence of demonstrable disease of the teeth or tonsils, the knitting of the fractured bone may be prevented by the injurious influence of pathogenic germs in an apparently healthy buccopharyngeal cavity. Such microbes have long been known to reach the lungs, the ears, the cerebral meninges, and other structures, where they may give rise to serious and sometimes fatal disturbances.

One of the reasons why harmful germs may lodge a long time in the mouth without causing trouble is to be sought in the physical integrity of the buccopharyngeal mucosa. But the normal standard of the mucous membranes of civilized man is easily disturbed by local traumatism, the devitalizing effects of cold, the inevitable neglect of the hygiene of the mouth in particular under the exigencies and privations of warfare. At the time of my visit to the hospital of the Indian Medical Service Corps in

Boulogne, in 1915, I learned that the natives at home are in the habit of cleaning their excellent teeth with small sticks of a special wood. In the hospital no less than sixty per cent of the men had pyorrhea alveolaris, undoubtedly due to neglected teeth, for toothbrushes were not accepted—as being made of pig's bristles, in other words, unclean according to their religious teachings—and difficulty was naturally encountered in supplying the special sticks to which the Indian soldiers were accustomed. This apparently trifling observation is extremely suggestive and in fact contains the key to some obscure problems of wound-repair in general and bone-repair in particular. Local susceptibility to microbic invasion must be supplemented by acceptance, or what Netter, of Paris, calls the consent, on the part of the body as a whole, in order to result in secondary infections and systemic disturbances. Here again, these conditions are admirably complied with by the lowering of the general resistance incident to the restrictions of hospital life. Fortunately, the degree of virulence of buccopharyngeal microbes is subject to fluctuations, the reasons for which are not yet entirely understood, and their capacity of interfering with the processes of tissue-repair varies in proportion.

When confronted with the task of restoring the integrity of a fractured bone, the surgeon therefore must learn to look at the patient as a whole, and to put him in the best possible general condition. A man used to an active, out-of-doors existence, with constant contraction and relaxation of muscle and the active oxidation-processes belonging to the same, suddenly finds himself stretched out in bed, or at best confined to the ward, with more or less loss of physical and mental tonus; all the cells of his body share in the suspension of work, with the result that resistance is lowered and defences are gradually sapped. Meanwhile, a polluted blood supply contaminates the seat of the fracture, and as the circulation is sluggish, tarries in the surroundings of the broken bone-ends, undermining their reconstructive energies.

The callus for the repair of the fractured bone is expected in a measure to form from the hæmatoma. Fractures do not consist merely of a broken and more or less displaced bone, but as we know, all bony injuries are associated with a bloody extravasate, which in closed fractures may remain sub-periosteal or pass into the surrounding tissue, or in open fractures escape to the outside. The loss of the extravasated blood has an undesirable influence upon the repair process, in so far as its presence undoubtedly favours the formation of the callus. On the other hand, aside from the danger of infection from without or within, the hæmatoma may exert an injurious pressure upon the neighbouring organs, causing congestion and lymph-stasis, which are further increased by muscular inactivity. This retardation of the blood- and lymph-current involves a more or less imperfect nutrition of the surrounding tissues and at the same time exposes them to the danger of *auto-infection*. The causative relation between the hæmatoma and the appearance of the callus is shown by

certain cases of clean open fractures in which the blood has escaped to the outside, with the result that a very weak callus is formed. On the other hand, it is a familiar fact that a sluggish callus-formation can be hastened through the injection of blood at the site of the fracture or through artificial production of a hæmatoma by rubbing the bone ends against each other.

The young callus at first has the character of granulation-tissue, but later on gradually increases in solidity through deposit of lime salts. Its *mechanical* properties are of great importance from the therapeutic view-point. Newly developed callus is semi-elastic and yielding, so that it becomes readily deformed under the influence of relatively slight forces, resuming its original shape after these forces have ceased to act. But the semi-elastic callus is unable to overcome a marked deformity which is apt to become permanent, the callus weakening and giving way under its influence. The frequent repetition of injurious factors, including irritation by endogenous pathogenic microbes and their toxins, acting upon the young callus, frequently results in a pseudoarthrosis, which means that no bony but merely a fibrous union has occurred. Provided all mechanical rules have been observed, pseudoarthrosis is probably referable to latent microbism in the majority of the cases. The solidity of the *exuberant* callus, which is sometimes formed as a result of latent infection, is due to its great bulk which counteracts its softness. When this mass later on undergoes a transformation into bone-substance, it acquires a much greater solidity than necessary. Before these superfluous bony masses can be reabsorbed through the osteoclasts, they may exert an injurious action through pressure upon adjacent structures, impairment of articular movements, or formation of ankylosis. In other cases the abnormal callus interferes with adjacent organs, especially nerves, resulting in atrophic changes. Exuberant callus will also develop when the two bone-ends are more or less displaced or imperfectly adjusted. After consolidation has taken place in these cases, the large callus serves to strengthen the bone, which always needs added support when its longitudinal axis is displaced.

The softness of the young callus requires fixation of the injured limb, at least to the degree that the slight restricted excursion does not exceed the elasticity of the callus. The *rest*, however, which is needed for the reunion of the broken bone retards the blood and lymph flow, while condemning the muscles to inactivity and exposing them to the danger of atrophy of disuse. As soon as possible during fixation, functional treatment (introduced by Lucas-Championnière) should be instituted in the form of exercises of the muscles, tendons and joints, so that their recovery may proceed along with the repair of the fractured bone. A very common cause of badly healed fractures of the long bones consists in stiffening of the adjacent joints, usually associated with incomplete correction of the displacement of the broken bone-ends.

It is not my intention to enter into a critical discussion of the various

methods of treating fractures, but rather to emphasize also in this connexion that the broken bone must not be considered exclusively, nor even receive the lion's share of professional attention. The muscles and joints must be given the benefit of early massage and judicious exercises, chiefly in the form of active movements in small excursions. A fracture is not to be considered as healed when the bone has anatomically united, but the muscles are damaged or the joints are stiff. The bad effects of immobilization in this respect led Dowden to the elaboration of the no-splint treatment of fractures. In cases of fracture of the humerus, for example, active movements of the fingers and wrist are encouraged from the first, together with pronation and supination, and the patient is instructed to flex and extend the elbow to a limited degree; three or four days later, passive movements are begun, aiming at complete extension of the elbow. It is certainly suggestive that in ten years he had no case of non-union or other untoward sequelæ. Ideal results can only be obtained by attending at the same time to all requirements, from correct position of the fragments and creation of the best possible physiological conditions for the muscles and joints, to the patient's general health and the removal of all foci of latent infections. The existence of an infectious focus at any point of the body often constitutes a serious obstacle to the healing of all fractures.

The mobilization of quiescent micro-organisms through changed circulatory conditions is very clearly brought out in a case of tetanus infection, observed in Germany during the war, as a sequel of artificial stimulation of the callus, for ununited gunshot fracture of the humerus, the injury dating back three and a half months. The flesh wound had healed and there was no other demonstrable lesion of the body. Four days after the loose fragments had been rubbed together, the patient was attacked by tetanic spasms, suggesting that tetanus spores had penetrated into the arm with the artillery projectile and remained viable for a long time between the bone-ends, until their development was favoured by the surgical traumatism. The most interesting feature of this case from our viewpoint is that this fracture had failed to consolidate, presumably as the result of the latent bone-infection.

The fact is not always appreciated that the complete repair of the broken bone does not necessarily coincide with a complete functional cure, but really does so under the most favourable conditions, whereas months or even years may intervene in other cases. The less the soft parts have been damaged, the more they have been exercised during the repair of the bone, the more likely are they to recover together with the bone. On the other hand, when the soft parts, especially the muscles, tendons, and tendon-sheaths, have been badly damaged, perhaps with involvement of adjacent joints, a functional cure is naturally delayed and may remain incomplete for a long time. In the great majority of cases, under adequate treatment, no further improvement of function is to be expected at the end of two or

three years. Certain sequelæ, more particularly chronic arthritis, may persist a long time, especially in those cases where latent microbism has found a favourable soil for colonies in the traumatized and non-resistant tissues.

There is reason to believe, moreover, that aside from auto-infection with buccopharyngeal microbes, a deficient or faulty *action* of *internally secreting glands* may be in part responsible for delayed union or non-union in fractures of the long bones. The endocrine glands undoubtedly exert some influence upon the bone-system, under physiological as well as pathological conditions. The growth of bone is known to be injuriously affected by the diminished or exaggerated function of certain ductless glands, notably the thymus, the thyroid, the parathyroids, and the supra-renals. The relation of the internal secretion of the ovaries to the health and strength of bone is well known, but may be left out of consideration in a review of endocrinic factors governing bone-repair in military fractures; nor is there reason to dwell upon the action of testicular hormones in these cases.

The existence of close relations between the *thymus* gland and the ossification-process has been repeatedly emphasized in the international medical literature. By Frontali in Italy (1920) the thymus gland of a child thirty-three days old, with congenital fragility of the bones and entire absence of the compact substance from the shaft bones, but with normal calcium balance and lime-contents of the blood, was found to be in a state of advanced sclerosis and atrophy. The other internally secreting glands were normal, so that he is inclined to attribute the deficient bone-development to the changes of the thymus and to recommend therapeutic injections with the serum of a child having a normal thymus gland, in cases where the function of this organ is deficient. Extirpation of the thymus in growing animals is usually followed by disturbances of ossification resembling rickets and leading to spontaneous fractures. The behaviour of young dogs with artificial fractures of the long bones was found to differ greatly, after the performance of thymectomy, from the behaviour of control animals of the same age and size whose thymus gland had not been extirpated. Whereas in the latter a strong callus, rich in calcium, made its appearance, the thymectomized animals developed only a very scanty and imperfect callus. Sometimes there was no demonstrable attempt at callus-formation at all, and a connective-tissue pseudoarthrosis developed instead. The effect of thymus-feeding upon artificially produced fractures in animals was recently (1918) investigated by Glaesner, with the object of ascertaining, in the interest of human surgery, if a more rapid repair of bone-defects can be secured by the administration of thymus substance. Rabbits of the same litter served for these experiments in which a circular defect of the tibia was produced by means of a trephine; some of the operated animals were then regularly fed with thymus gland substance, others which received no

thymus serving for controls ; all were kept on exactly the same diet. The size of the bone-wound was ascertained once a week by X-ray pictures. After a certain time (four weeks) the animals were killed, and the tibias of the treated and normal rabbits were examined and compared, with the result that very striking differences in favour of the former were noted. These findings undoubtedly suggest a beneficial influence of thymus administration upon the repair of bony lesions, and it seems not improbable that these experiments may acquire practical importance for the prevention of delayed union in fractures of human shaft bones.

The marked nutritional disturbance set up in the organism as a result of *thryoid insufficiency*, partly in the form of incomplete decomposition of the albumins, partly in the form of deficient oxydation of the fats, was discussed several years ago by Hertoghe of Antwerp, and he emphasized the fact that all tissues of the body are affected by nutrition, or malnutrition, respectively. In the presence of thyroid insufficiency, no tissue remains unchanged, and the bones are no exception to the general rule. The administration of thyroid products is followed by extremely favourable results in cases of this description. The favourable effect of thyroid preparations upon the repair process after fractures is known to be due to the resulting acceleration of the deposit of lime-salts. A French observer, Gauthier, a number of years ago, published observations on cases of delayed union and pseudarthrosis, cured by a fortnight's administration of altogether six to ten grammes of fresh sheep's thyroid, and these findings were confirmed by several other investigators. In thirty-two of thirty-seven cases of delayed union, the fracture became promptly consolidated, in Gauthier's experience, evidently due to the stimulating effect of the thyroid extract upon the osteogenetic tissue. During the repair-process, a large callus developed, which underwent retrogressive changes when the thyroid medication was stopped. Fractures of the long bones are said to have healed within three weeks (seventeen or eighteen days). Gauthier accordingly recommended the routine administration of thyroid extract, from the start, in the treatment of fractures. Animal experimentation has shown that removal of the thyroid gland, after a fracture has been inflicted, leads to delayed consolidation of the bone in practically all cases without exception.

The *parathyroids* in their turn are also known to exert a regulating influence upon the calcium metabolism. By some observers, the tetany which follows upon extirpation of these small bodies has been referred to a deficiency in calcium in the organism as a whole. Leaving aside the tenability of this theory, the fact that interests us here is the potential part played by functional disturbances of the parathyroids, as a result of undernutrition or other causes, in the complicated process of bone-repair.

The action exerted by the *suprarenals* on bone-repair probably constitutes a highly intricate process. Stress and strain, physical as well as mental, which after all are the conditions under which most fractures are

inflicted, through their effect upon the nervous mechanism of these glands presumably diminish the output of sugar into the blood and thereby reduce its bactericidal powers. The impairment of the glycogenic function and the chemical alteration of the blood inviting intravascular and intracellular infection, the invading bacteria are no longer prevented from penetrating into the depth of the traumatized tissues, and the metabolic dyscrasia ultimately results in delayed bone-repair.

The direct beneficial influence of *adrenalin* upon bone-repair and callus formation was pointed out several years ago, by Carnot and Slavu, who in their animal experiments observed a better bony consolidation in dogs which were given a daily subcutaneous injection of six milligrammes of adrenalin, as compared to control animals of the same age and weight whose tibial fractures were allowed to heal without adjuvant adrenalin medication. Even to the unaided eye, the favourable influence of adrenalin upon the process of bone-repair was clearly apparent, and these findings were confirmed by microscopical examination. There seems to be a reaction on the part of the bone-marrow on the one hand, while on the other there is an increased deposit of lime-salts under the influence of the drug.

The *cerebral hypophysis* or pituitary gland is known to affect the osseous system, a disordered function of this organ leading to the changes of giantism, dwarfism, and acromegaly. Opinions are still at variance, however, as to the manner in which this influence is exerted. Experimental injection of pituitary extracts in particular has yielded variable results in the experience of different investigators, in the form of delayed or hastened bony growth, increased or diminished excretion of calcium. Just before the outbreak of the World-War in 1914, Marsiglia, in Italy, published investigations concerning the effect of the active constituents of this organ upon the healing of fractures and the formation of the callus. The outcome of his experimental intramuscular injections of a preparation of fresh whole calves' hypophysis into dogs, whose femurs had been fractured on the day of the injection, was not only negative as regards the acceleration of the repair process by pituitary medication, but he was actually enabled to ascertain a delayed callus formation as compared to the control animals.

In this connexion, mention may be made of *gelatine*, a product derived from bony, cartilaginous and connective tissues, which was originally and very successfully employed to promote the consolidation of fractures, by Anzilotti of Pisa. A sterilized solution of gelatine (2·5 per cent) is used together with sodium chloride (0·75 per cent), phenol (0·50 per cent) and calcium chloride (0·50 to 1 gramme per cent). A dose of ten cubic centimetres of this solution is injected daily or every other day, into the muscles or under the skin. The results, in the form of improved healing of fractures, are claimed to be uniformly excellent, without untoward sequelæ of any kind.

Upon the basis of these experiences, as briefly sketched in the foregoing remarks, the question naturally arises if the administration of carefully selected endocrinic extracts, known to exert a stimulating effect upon bone-growth and ossification, might not be advantageously introduced as a routine feature into the treatment of fractures of the long bones. In all probability, the normal function and co-operation of several internally secreting glands is essential to the utilization of the calcium and phosphorus contained in the food, in the metabolism of the bones. At the present limited stage of our knowledge, the subject may be tentatively envisaged from two viewpoints, referring the disturbances in the repair-process either to general malnutrition, with involvement of the endocrine glands, or possibly to the absence of the indispensable vitamines, damaging the function of these ductless organs.

Vitamines are nitrogenous crystalline bodies of very complicated structure, existing in fresh animal and vegetable foods. Aside from general nutritional disturbances due to the absence of vitamines from the diet, peculiar systemic disturbances of the bony framework of the body have recently been described in Germany and in Austria, where they assumed an endemic character. Much attention has of late been devoted to diseases due to restriction of fresh animal and vegetable foods, with absence of vitamines from the diet, and these conditions are sometimes grouped together under the heading of "avitaminoses," comprising scurvy, beri-beri, pellagra, infantile scurvy, rickets, and osteomalacia. The vitamines, aside from their absence in unsuitable foods, may be destroyed by mechanical or chemical processes or through the action of excessive heat. In experiments upon young guinea-pigs and other animals, not only scurvy-like conditions but actual changes of the bones have been produced through exclusive feeding with highly sterilized cow's milk. Dietetic errors in the form of a deficiency of calcium in the food are naturally capable of inducing an impoverishment of the bone in lime. Disturbances in the phosphorus metabolism also enter into consideration, for it has been shown experimentally that the bones may become decalcified through continued administration of food poor in phosphorus, leading to osteoporotic changes, proliferative processes in the periosteum and cartilages, thickening of the epiphysis, and curvatures of the bones.

Empirical proof of the favourable influence of fat soluble vitamines on the growing and mature osseous system has long been furnished, through the beneficial effect of codliver oil on rachitic children, this substance being extremely rich in these vital supplementary food constituents. Comb honey also has been shown to contain a moderate amount of fat soluble vitamine, but this is entirely lost in the commercial strained product. Denatured and so-called predigested foods, if not positively injurious, undoubtedly deprive the body of important building material and are probably in part responsible for incomplete reconstructive processes of various kinds. The harmful action of defective foods on the teeth, and their part in the

production of caries, has often been discussed in the dental and orthodontic literature. Through an unsuitable diet consisting entirely of soft starchy foods, disturbances in the form of osteoporosis have been experimentally produced in the growing bones of young dogs ; the absorption of the bone-substance was increased and the formation of new bone was diminished, as compared to the controls.

The same factors which are concerned in the non-traumatic diseases of the bones and joints are likewise operative in bone-repair after fractures. Blood chemistry no less than blood bacteriology enters into consideration in the complicated repair process of fractured bones, and the significance of chemical factors must not be lost sight of in this connexion, for the normal course of bone-repair is often arrested by chemical change without bacterial invasion. All fractures are accompanied by a lowered local vitality, due in part to a changed blood-supply and diminished nutrition.

Absence of the normal bone-salts is demonstrable in many cases, and this departure from the normal standard is visible to the experienced eye in X-ray plates of the affected bones. My own experience includes cases in which no callus had formed although the mechano-therapy could not be improved upon. Without any other modification of the treatment, these patients were given calcium salts for a variable length of time, according to requirements, with the result that callus-formation and union were promptly obtained. One such case is at present under my care in the Naval Hospital and the healing of the fracture is making satisfactory progress.

The value of calcium triple phosphate as a stimulus to osteogenesis has been pointed out by Albee and Morrison, in whose experience the injection of one cubic centimetre of a five per cent solution into the bone-defect of rats, on the third day after the infliction of a fracture, was followed as soon as five days later by the beginning of callus-formation shown by X-ray pictures, with union of the bone-ends at the end of two weeks, whereas in the controls the defect was not yet bridged over after thirty days and only a slight callus formation was demonstrable.

Although ten years have passed since the bearing of focal infections on bones and joints was first pointed out by Hunter in London, this subject is only gradually receiving the wider attention of the medical profession. Judging from reports in the literature, opinions are still about equally divided as regards the ætiological rôle of mouth infections in bone and joint metastasis, for example. However, the existence of latent microbism and auto-infection is generally conceded, and the fact is freely admitted that disease is not exclusively due to agents introduced from without. What remains to be enforced is the application of this principle to the course of repair-processes in the body in general and in fractures of the long bones in particular. The defensive mechanisms of the body must be constantly on the alert in the prevention of exogenic as well as endogenic infections.

Sir Almroth Wright, in 1919, in his memorable address before the Royal Society of Medicine, entitled, Some "New Prospects in the Field of Therapeutic Immunization," offered a new explanation applicable to the lodging of bacterial emboli in bones and joints. "A region of the body in which the guardian elements of the blood have been rendered impotent, or as the case may be, have been excluded," is designated by him as an ec-phylactic region, and such a region may be induced by *any influence which interrupts* or closes down the *arterial supply* to the part. A fracture would seem to provide an ec-phylactic region *par excellence.*

The modifications of the regional blood-supply incident to a break in the continuity of a given bone naturally play an important part in the repair of the lesion. Injury of the nutrient artery of the broken bone is often an inevitable concomitant of all fractures. A number of delicate new blood-vessels are formed in Nature's effort at compensation, for the maintenance of a sufficient blood-supply. When the extension of the contracted parts is not very carefully timed and manipulated, the stretching of all the regional blood-vessels results necessarily in diminution of their calibre and nutritional impairment of the area supplied by them. A highly injurious influence may thus be exerted upon the constructive metabolism of the bone. In order to obtain the best results in the treatment of fractures, practical application must be made of the war-taught lesson that the *normal* circulation, nutrition and mobility of the injured limb must be maintained as far as possible, thereby ensuring the vitality of the parts and favouring the formation and solidification of the callus, while guarding against the wasting of muscles and the stiffening of joints.

The many concealed factors at work in the complex process of bone repair which are entitled to serious study in behalf of improved results of fracture-treatment are only beginning to be understood and appreciated, so that a promising field is thrown open for further investigation along these lines. The points touched upon in this review may be briefly summarized as follows.

Factors influencing bone-repair after fractures:—

(1) Blood bacteriology:
- (*a*) Lowered resistance of fractured bone, favouring auto-infection and deposit of infectious agents from the blood (latent microbism).
- (*b*) Metastatic infection of fractured bone, from concealed foci in the tonsils, teeth, gastro-intestinal or genito-urinary tract.
- (*c*) Constitutional disease, such as syphilis affecting the structural metabolism of bone-tissue.

(2) Blood chemistry:
- (*a*) Action of internally secreting glands.
- (*b*) Metabolic dyscrasias.
- (*c*) Effect of vitamines, changes in phosphorus and calcium metabolism.

(3) Interference with the constructive metabolism of bone through injury to the nutrient artery and changes in the calibre of the regional blood-vessels.

Diseases are often called functional, merely because no organic basis for the existing disturbance can be demonstrated, but this apparent absence of course does not exclude the possibility of an organic lesion as the source of the symptoms. Modern experience teaches that toxic-infectious processes are to be reckoned with in numerous obscure conditions, and delayed bony union after fractures, for which no satisfactory explanation can be found, undoubtedly is one of these conditions. It may be that the injured bone is affected by minute vascular changes, as the invisible anatomical expression of an endogenous infection or intoxication. The solving of these questions must be left to future investigations, by laboratory methods and animal experiments. But the time has arrived for all surgeons to realize the fundamental therapeutic importance of the problem of a possible endogenic cause or metabolic dyscrasia for delayed union after fractures. Convinced as I am of the necessity for more accurate study along these lines, my endeavour has been to point out these new vistas in fracture-treatment, by inviting attention to some factors in bone repair.

AN INTRODUCTION TO THE STUDY OF HÆMATOPHAGY.

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(*Continued from p.* 341.)

II.—HÆMATOPHAGY AS A PATHOLOGICAL OCCURRENCE.

We have seen that the lymphocytes of the guinea-pig customarily devour red corpuscles, but, unlike the macrophages, are unable to assimilate this food, only altering it into a Kurloff-body. Nevertheless, the fact that these cells may be hæmatophagic immediately suggested to my mind the possibility of certain other types of cells—other cells than blood-cells—behaving similarly, *under abnormal conditions.* The Kurloff-bodies have often been compared, in a general way, with Guarnieri's bodies, Negri's bodies and similar formations; I mean, as being not necessarily identical, but of the same order of enigmatical structure. I realized that there was nothing inherently improbable in the idea of a tissue-cell, under the influence of some powerful, exciting stimulus, such as a virulent toxin, reverting to the primitive mode of behaviour found, for instance, in a large number of unicellular animals (Protozoa), that, namely, of ingesting "solid" food.

The Negri-bodies.

The Negri-bodies have been so well described that not much need be said about them by way of introduction. They are characteristic inclusions occurring in cells of the nervous system in hydrophobia. They occur commonly in the Purkinje cells of the cerebellum, and especially in the cells of the *cornu Ammonis* (*Hippocampus major*). Readers may be particularly referred to Negri's own works [8 and 9]. As to their nature, the "Chlamydozoan" view is very widely held. This is, that the conspicuous bodies themselves represent extruded nucleolar matter, enveloping the actual minute, parasitic elements. The usual complicated type of life-cycle has been described.

Acton and Harvey [1], alone among recent workers, so far as I am aware, take the view that the Negri-bodies are the result of an extrusion of nucleolar matter, in response to the exciting stimulus, but not in connexion with any parasite; that is, that no parasite is concerned, the "extrusion being the result of katabolic changes in the nerve-cell caused by the action of the rabies-virus." But the chief merit of the work of these authors is that they have been able to find bodies similar to Negri-bodies, in the nerve-cells of animals subjected to *various other stimuli*, such as viper-venom, emulsion of *Bacillus pyocyaneus*, and so on.

Among my preparations of the Negri-bodies are certain which were kindly given by Negri himself, some years ago, through the instrumentality of Dr. Visentini. One of these is an ordinary Giemsa-smear, and I will describe this first, as the appearances to be observed are exceedingly instructive. It must be borne in mind that the smear is more or less a "mush" of cell-substance. There is a common matrix, which consists of the cytoplasm of the cells which has run together, as it were, so that their individual outlines are usually indistinguishable. The smear itself, however, is very well fixed and stained. But it follows that, from the smear alone, it cannot be said definitely what was intracellular, and what was extracellular; where I indicate which, in my opinion, of the two conditions was the case with respect to any particular body, I am having regard also to the situation of the corresponding forms, as seen in sections.

Immersed in this ground-substance are various organellæ and bodies of perfectly definite nature. Narrow blood-capillaries (fig. 13*a*), of varying length, traverse it in different directions; these end abruptly, with open ends, and have probably been broken in making the smear. These capillaries contain more or fewer red corpuscles, which appear yellow (unstained). The corpuscles are often flattened laterally and slightly elongated, owing to the narrowness of the capillary; moreover, the adjacent ends of two corpuscles, in contact, are often indistinguishable. Corpuscles, identical in appearance, also occur free throughout the matrix itself. Scattered about are the neuroglial nuclei and also Negri-bodies, the latter being more common in some places than in others. They are of varying form, size and appearance.

A regular series of transitions can be found between what are undoubtedly red corpuscles, or corpuscular masses, and fully formed Negri-bodies. In the first place, quite short segments of a capillary are occasionally seen, narrow as usual, but completely rounded off at each end, by the delicate enclosing wall, which stains always red. In these isolated segments, the enclosed corpuscles are stained a greenish-blue, with perhaps a tint of yellow; that is to say, some alteration is already occurring in such cases which tends to produce a kind of polychromatophilic staining. In addition, ovoid masses occur (fig. 13), scattered about, varying in size from that of a single corpuscle to that corresponding to three or four, indistinguishably united. These masses all stain in a greenish-blue manner and represent equally blood corpuscles or corpuscular masses. In the same field may be seen such bodies and also normal, unaltered yellow corpuscles. The larger masses are invariably surrounded by a delicate red-staining sheath; there is no question of this being merely a deposit of stain, because it is never present around free, unaltered corpuscles. Occasionally, these bodies are narrow and elongated. In some of the masses, fine granules, faintly red, are present, even while there is still a tint of green in the general staining; in others, the granules are much more prominent and stain more deeply and the substance of the altered corpuscles is definitely light blue.

In one case, of a fairly large ovoid mass, with well-marked granulations, most of the ground-substance is blue, but near one end there is a pure yellow area, i.e., a still unaltered corpuscular fragment, incorporated in the mass (fig. 13*b*). Lastly, in others again, discrete, usually round, light-red staining areas are developed in the blue substance; there may be only one or two, or in larger bodies several of these.

Thus, finally, the stage of the fully-formed Negri-body is reached, as depicted by Negri from Giemsa-smears (fig. 13*d*); the larger, homogeneous, light red elements correspond to the "grosse Innenformationen," the small, dark red, mostly more superficial elements are the "kleine Innenformationen."

Bearing in mind the above reservation, I consider, nevertheless, that not only the polychromatophilic alteration of the corpuscles, but the whole transformation into a Negri-body, may, on occasion, take place really extracellularly, while the blood is still in a narrow, capillary segment. In one instance in a segment about ten times as long as broad, i.e., much longer and narrower than any ovoid mass I have found in a cell (in sections), rather less than half is occupied by an oblong, faintly yellow mass, representing probably two corpuscles: the other, larger half consists of a long, cigar-shaped Negri-body, with five of the larger inclusions (fig. 13*c*). As noted, many of the developing Negri-bodies have this elongated shape; and I think this is to be accounted for by their origin from short, broken capillary segments, the blood in which has been altered *en masse*. How these short portions of capillaries have been broken or separated I cannot say; but I consider that the larger, ovoid masses (definitely intracellular as seen from sections), result from the comprehensive ingestion of such short segments, the pink-staining sheath being probably the delicate wall, and the whole being moulded by the cell into a more or less ovoid shape. Where alteration, to a greater or less extent, takes place extracellularly, it must be inferred that the ferment can be poured out of the cell.

In sections stained by Mann's method, the preparation also being one which was kindly given by Negri, the red corpuscles, here again, are often compressed and tend to be crinkled lengthwise; thus they have a remarkably refringent appearance (fig. 14A, *c*). They stain a bright yellow-pink. It is important to note that isolated corpuscles—quite separate from a capillary—can be found without difficulty. These are undobtedly *in situ*, and their occurrence free points to disorganization of some of the capillaries. Small Negri-bodies are just about the same size as a corpuscle and are also often *similarly elongated*. These small bodies now and again show one or two colourless, rather refringent elements inside them, these inclusions being the so-called "vacuoles." In the larger ovoid bodies, these inclusions are more numerous and some of them larger. The colour of the whole Negri-body is a bright red, lighter in the smaller ones and darker (stronger) in the larger ones. An elongated corpuscle becoming altered into a Negri-body can be distinguished from an unaffected corpuscle only by its slightly different staining appearance, unless a small clear area has already been formed. On the other hand, the nucleoli of the nerve-cell nuclei are all round, uniform in size and of the same shade of purple, this colour being quite different from that of the Negri-bodies. (A very similar picture is shown in Muir

and Ritchie's "Manual of Bacteriology," 1913, pl. 4, fig. 16, from material given by Harvey and stained with methylene-blue and eosin.)

Lastly, I compared some sections made by myself which were stained by iron-hæmatoxylin and eosin. In these, the iron-hæmatoxylin has been almost completely extracted from the Negri-bodies which are uniformly stained with the eosin, excepting where the pale inclusions stand out (fig. 14B). But around these inclusions there is often the last trace of hæmatoxylin in the form of a fine greyish-black rim. That is to say, by this method of staining *the appearance of the Negri-body and of the Kurloff-body is essentially similar.* The nucleoli, on the other hand, are always an intense black; i.e., they *retain* the iron-hæmatoxylin much more firmly and suggest an organella of entirely different character, namely, a karyosome, or chromatin-containing nucleolus. (The nuclei themselves of the nerve-cells seem to be very poor in chromatin.)

There is not, in fact, the slightest evidence in my sections suggesting any connexion between the nucleolus and the Negri-body. None of the nucleoli show the least signs of activity. They are uniform in size and appearance, both in cells containing a Negri-body and in cells which do not; and, as remarked, *whichever way they are stained* they are quite different from the Negri-bodies. Even if I had not been able to demonstrate actual stages in the alteration of corpuscles into a Negri-body, the nucleolar-extrusion hypothesis would have, I consider, nothing in its favour, whether regarded as a cloak to hide a "Chlamydozoan" or not.

It will be apparent that these varied appearances can all be readily correlated with those shown by the Kurloff-bodies when correspondingly stained. It must be remembered, however, that the hæmatophages concerned in the two cases are, respectively, cells of quite different order; in the one case a nerve-cell, in the other a lymphocyte. Consequently, it may well be expected that both the exact nature of the ferment secreted and the alteration produced in the ingested corpuscles as a result of the attempted digestion will be rather different in the two cases.

To take first the main points of agreement in both cases, only red corpuscles are eaten. *The fundamental structure of both bodies is the same* and consists of two chief components: (1) A homogeneous substance, filling the body, in which are included (2) elements, or "large inner formations," which are of varying size but usually round, in the case of Negri-bodies, and not only of varying size but of most varied form, in the case of Kurloff-bodies. In smears stained by Giemsa, these inclusions are a light, bright red in the Negri-bodies, and a rather darker red in the Kurloff-bodies. In films or sections, on the other hand, the inclusions remain colourless (unstained) in both cases.

The chief difference is due to the iron-compound, separated from the protein elements, being in a somewhat different condition in the two cases. In the Kurloff-body, it is in a more liquid condition, and constitutes a practically spherical globule. In the Negri-body, the corresponding substance is more solid and may retain the shape of the original corpuscle, or corpuscular mass from which it is formed, until it is moulded by the

hæmatophage into an ovoid shape. Further, whereas in the Kurloff-body, this substance stains a rose-colour by Giemsa, it stains a light blue in the Negri-body; i.e., it corresponds more to the polychromatophilic type of staining of hæmoglobin, in an altered condition.

In addition to the two principal constituents, granulations occur in the Negri-body (the "small inner formations"), but they are only conspicuous in Giemsa smears, doubtless rendered unduly prominent by this stain. When considering the Kurloff-bodies, I mentioned that occasionally, in smears where the contents of the "vacuole" have been retained (see p. 336) I have observed corresponding granules, in addition to the uniform, rose-coloured mass.

The varying size of the Negri-body depends entirely on the amount of corpuscular material ingested by the nerve cell, just in the same way as does the size of the Kurloff-body. *There is no growth*, or increase in size *per se*. The smallest Negri-bodies result from the ingestion of a single corpuscle (or even a fragment of one—just as a large mononuclear, or a lymphocyte may take up a portion of one); the largest, from the comprehensive ingestion of a mass of three or four corpuscles; and in one and the same cell there may be examples of both (cf. fig. 14B with fig. 11).

Here, too, it would appear that the hæmatophage is unable to assimilate this altered but only partially "digested" food. In my preparations I have not found the later stages depicted by Negri. From these, it is most probable, however, that the substance of the Negri-body breaks up into small fragments, "gemmules," which are ultimately dissipated—it may be when the nerve-cell is killed, or they may be expelled.

THE "CHLAMYDOZOA."

It is seen, therefore, that under the influence of the violent stimulus occurring in hydrophobia, even such highly specialized cells as nerve-cells may become hæmatophagic. Now, Guarnieri's bodies, occurring in the epithelial cells in small-pox and vaccinia, are *generally agreed to be bodies of similar character and to originate in the same manner as the Negri-bodies*. They, equally, have been included in the "Chlamydozoa," under another imposing name. I have not myself actually studied Guarnieri's bodies, but there can be, I consider, no doubt whatever, that these, too, are simply the result of epithelial cells becoming hæmatophagic,[1] with consequences of the same order; i.e., alteration, but not digestion and assimilation of the ingested material. Some of the "phases" described, whether intracytoplasmic, as in vaccinia, or intra-

[1] It must be noted that I say "hæmatophagic," i.e., blood-eating; it may well be that the epithelial cells do not restrict themselves to red corpuscles, but eat leucocytes also. In that case the alteration of the nuclear material would give a somewhat different result again. It is significant that various workers have already thus explained Guarnieri's bodies, e.g., Ewing who regarded them as derived from red corpuscles, and Ferroni and Massari and also Salmon, who derived the inclusions from leucocytes.

nuclear, in small-pox, are almost identical in apppearance with Negri's bodies and are certainly to be regarded as similar formations.

And the same explanation may also apply to the case of the bodies in "*molluscum contagiosum*," trachoma, Mallory's bodies in scarlet fever, etc. In short, I wish to express my considered opinion that Prowazek's entire conception of the Chlamydozoa, with their "elementary corpuscles," "initial bodies" and all the complicated phases of their life-cycle, will be proved to lack reality and must be banished as an illusion. At any rate, there are no such parasites or parasite-complexes as "*Lymphocytozoon cobayæ*," "*Leucocytozoon* (*sic !*) *syphilidis*" and many of the intracellular "phases" intercalated by certain authors into the life-cycle of *Treponema pallidum*, "*Neuroryctes hydrophobiæ*" and "*Cytoryctes variolæ* and *vacciniæ*."

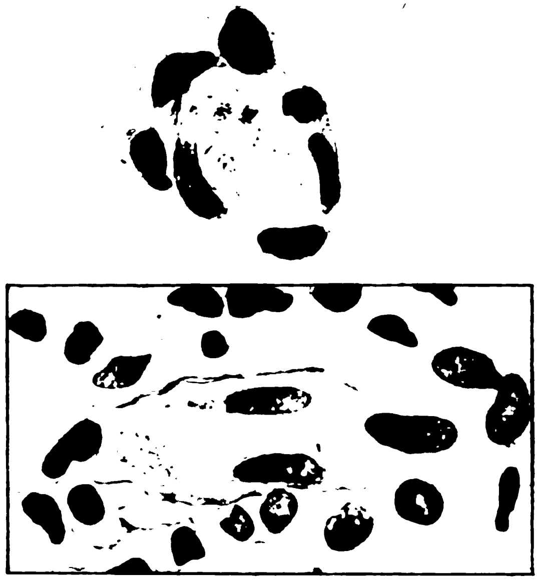
The "Rickettsia"-Bodies in Typhus, etc.

Lastly, I wish to suggest that the parasites which have been described under the name of *Rickettsia*, occurring in typhus, and the very similar ones known as *Dermacentroxenus*, in Rocky Mountain spotted fever, a closely allied disease, are also to be regarded *as granular formations produced as a result of hæmatophagy*; in other words, that the minute bodies hitherto found in cases of these diseases and placed in the above category have, in reality, nothing of a parasitic or organismal nature about them.

I have not worked at these bodies myself, but as a result of my study on hæmatophagy I am in a position to put forward certain reasons which seem to me to indicate a strong likelihood of the view now advanced; these reasons are suggested by consideration of some recent papers dealing with the subject of typhus, or typhus-like diseases, and the Rickettsias, and I think they are worth serious attention and investigation on the part of those interested in this question.

In the first place, it is important to remember that the essentially characteristic lesion in this type of disease is not one of the skin, or of nervous tissue, as in the case of the "Chlamydozoan" diseases above referred to; *it is one of the vascular tissue itself*. Thus Wolbach, in his account of Rocky Mountain spotted fever (*Journal Medical Research*, 41, 1919, p. 1) states as follows: "The lesions of the blood-vessels are due to the presence of the parasite and constitute the distinctive pathology of the disease and warrant the definition, 'an acute specific, infective endangiitis, chiefly of the peripheral blood-vessels.'" "The lesions are at first essentially proliferative (endothelium), followed by necrosis of small groups of cells, and the chief cellular reaction, both locally in response to the presence of the parasite, and in general, presumably in response to toxins, is endothelial." Again, Wolbach and Todd, dealing with *typhus exanthematicus* in Mexico (*Ann. Inst. Pasteur*, 34, 1920, p. 153), write as follows: "La lésion consiste essentiellement dans une réaction de prolifération de l'endothélium vasculaire, bientôt suivie d'une infiltration de leucocytes

polynucléaires dans la paroi cellulaire . . . La présence des cellules mononucléaires que l'on observe, en migration dans les parois des vaisseaux sanguins, nous amène à cette conclusion que les cellules mononucléaires des infiltrations périvasculaires sont d'origine endothéliale." Finally, Stevenson and Balfour, in their recent summary of the histo-pathology of typhus (*Journal Path. Bact.*, 24, 1921, July, p. 289), say that it is now recognized that typhus is really a systemic disease of the smaller arteries and capillaries and that the vessel-changes are not limited to the skin, but occur in all the viscera.



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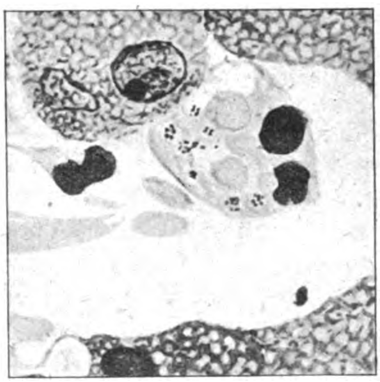
Further, just in the same way as the relatively large "parasitic formations" are associated with the chief foci of the disease in the case of "Chlamydozoan" diseases, e.g., Guarnieri's bodies in the epithelium, Negri's bodies in the nerve-cells, etc., so in typhus, the minute bodies which have been found hitherto and regarded as the causative organism occur mainly in cells of the vascular tissue—endothelial cells. Thus, Wolbach, in addition to the first sentence above quoted, speaks of the remarkable specificity of the parasite for the peripheral blood-vessels, and throughout his case-reports refers frequently to the finding of numerous parasites in endothelial cells and large mononuclears. Again, Wolbach and Todd say that, in Mexican typhus, " ils (les parasites) apparaissent en larges placards localisés exclusivement dans les cellules endothéliales " (cf. text-fig. 1). And Kuczynski found large aggregations in the Kupffer (endothelial) cells of the liver, in cases of typhus.

Now, in my opinion, certain remarks of Stevenson and Balfour are most suggestive in this connexion. In the first place, these authors are undoubtedly hesitating whether to regard, as *Rickettsia*-parasites, certain red-staining granules, which they themselves persistently refer to as "platelet-like bodies" or as "similar to blood-platelets," occurring in the phagocytic cells of the spleen-pulp, in those of the liver (endothelial or Kupffer-cells) and elsewhere. It is true they seem rather more inclined to pin their faith to certain blue-staining granules (often diplococcal in form) occurring in the walls of the vessels in the brain and lung, but they conclude as follows: "Although we have never found in the Kupffer-cells of the liver, the large aggregations of *Rickettsia*-bodies described by Kuczynski, it is possible that the platelet-like bodies above-mentioned are really of this nature. Here, as also in the spleen, they stain with Giemsa in the same way as do the *Rickettsia* of Kuczynski, taking on a red colour. Hence they present a contrast to the bodies found in the brain and lung."



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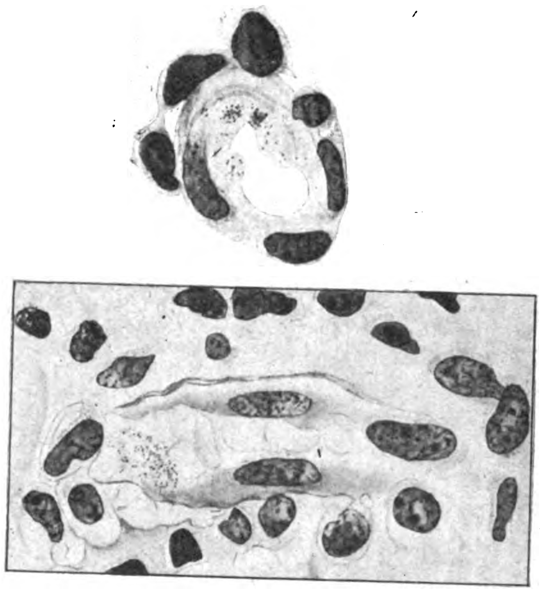

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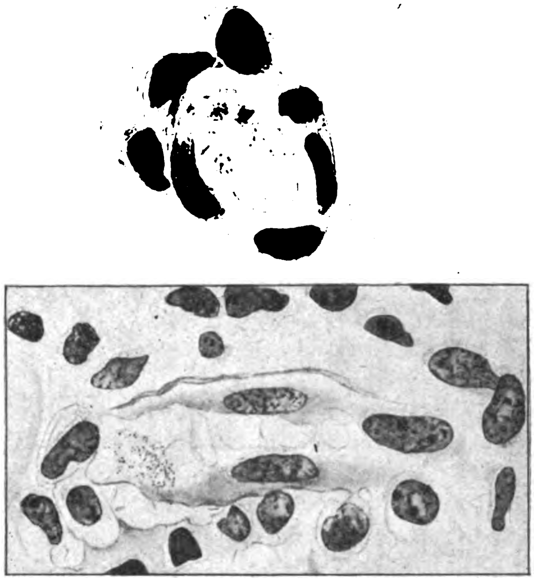

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containing both "small masses of red-staining granules resembling blood-platelets," which correspond most probably to Rickettsias in the same situation, *and ingested red corpuscles.*

What I wish to suggest, therefore, will be, I think, apparent. It is that the granules hitherto described as *Rickettsia are essentially nothing more or less than platelet-granules, a by-product of the digestion of red corpuscles and, it may be, of leucocytes as well.*

In typhus-diseases, the virus attacks and stimulates (at any rate, at first) the endothelial cells; stimulates them to hæmatophagy, just as the viruses of smallpox and hydrophobia stimulate the respective tissue-cells attacked to the same mode of behaviour. In the case of typhus, however, we should *not* expect conspicuous "bodies," like Guarnieri's bodies and Negri's bodies, to be formed, as a result of an *unsuccessful* attempt to digest this food. The normal macrophages (megakaryocytes and large mononuclears) belong to the endothelial cell-type, from which they are derived. Therefore, on *a priori* grounds, if an endothelial cell, in a situation where it is not normally hæmatophagic, becomes nevertheless stimulated to ingest blood elements, it is to be expected that digestion will be fairly complete and successful. *We should expect*, in short, something comparable to platelet-granules and platelet-cytoplasm to be produced.[1] And, as the former, I would explain the "*Rickettsia*"-bodies which have been found up to the present.

These products of digestion by the endothelial cells may not be identical with those normally produced in the spleen and bone-marrow by the customary macrophages; indeed, in one important respect it seems to me there may be an intrinsic difference, to which I refer below. In this connection, I am reminded that when Captain Horne, I.M.S., and I were studying blood-smears from Egyptian typhus and relapsing-fever patients at Suez, we came across, on two or three occasions, rounded cytoplasmic masses containing no nucleus, but numerous dispersed, red-stained granules; these bodies perplexed us greatly. We came to the conclusion that they occurred only in the blood of typhus cases, because we never found a relapsing-fever spirochæte in smears in which these bodies were present. For a short time we queried the possibility of their being some "parasite"; but even then we came ultimately to the conclusion that these cytoplasmic masses (which were much larger than any individual platelet) had been abstricted from cells of large mononuclear type. In the light of my recent work, I have no doubt now that these masses represented platelet-cytoplasm, formed in a local endothelial lesion.

On this view the number of platelets, or platelet-like bodies, may be expected to be increased to some extent in typhus. I am not aware of any

[1] Whether there is much or little production of actual "platelets" will depend (inversely) on the extent to which multiplication as a whole (proliferation) of particular cells takes place.

paper giving actual platelet counts; the only reference I have is to a paper by Cazeneuve (*Bull. Soc. Path. exot.*, 13, 1920), in which the author remarks that "leur nombre était considérablement augmenté."

Readers interested in this subject will know that there is by no means general agreement on the part of workers as to the cause of typhus and typhus-like diseases. Even as regards the Rickettsias, Stevenson and Balfour, in their paper published only last July, "confess to having doubt as to their exact nature, let alone their pathological significance"; and they are not the only ones. Now, as regards the Rickettsias found in insects, I do not wish to say anything about these; and I leave the question of their nature and significance entirely to my confrères at the Lister Institute, who have done the most valuable work in this connexion. All that I have been concerned to do here is to give my reasons for considering that the alleged parasites of this type hitherto described from cases of the above diseases are much more likely to be products of hæmatophagy. And my point is that, if such is the case, *nothing* has been so far detected in the human body which can be regarded with any degree of certainty as the causative organism. In other words, in typhus as in the "Chlamydozoan" diseases, we are apparently still as far as ever from unmasking the elusive micro-organism therewith associated.

Hæmatophagy as a Possible Cause of Disease.

I know quite well that in all these cases it can be said that there is, nevertheless, some ultra-microscopic, practically invisible organism, or organismal phase, present. But I will ask readers to dismiss from their minds for a moment all thought of a living, specific micro-organism in this connexion, and consider whether there is not another possibility with regard to the virus.

The various formations, bodies, etc., to which reference has been made, all occur in cases where, apart from the question of themselves, no microbic cause has yet been definitely ascertained. On the other hand, so far as I am aware, with one exception,[1] no corresponding bodies have been described in known microbic diseases, at any rate, as of general occurrence and on any important scale. Whereas these formations, if not in all cases absolutely specific, are nevertheless found so constantly as to be diagnostic. In themselves, these bodies are to be regarded as nothing more than the results of an attempt to digest red corpuscles (and sometimes, probably, other blood-elements also). But it can hardly be doubted that they signify much more than that; namely, that these formations *stand in some fundamental pathological relation to the disease.* And I venture to think they may nevertheless hold the secret of the ætiology, although they are not parasites.

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[1] Namely, the formations which have been described in connexion with syphilitic lesions, by E. H. Ross and others (see also below).

What is necessarily associated with the production of these bodies? There must be some ferment or enzyme secreted, to begin with, causing the alteration in the composition of the ingested material which becomes manifest. Secondly, there will be certain products of the interaction formed which may be regarded as being of the nature of waste or excretory substances, unusable by the cell. Here, the domain of Bio-chemistry is entered, and it is in the hands of the Bio-chemists rather than in those of the Bacteriologists that I think the elucidation of the cause of these diseases will lie. I am only able to indicate, roughly, the general idea that is in my mind.

In the first place, may not some proteid substance formed as a result of this unusual metabolism be actually harmful to the organism; in other words, itself assist in giving rise to some of the symptoms of the particular disease? The formations themselves, or the contained elements in the case of conspicuous "bodies," or, ultimately, the products of their degeneration and disintegration, may constitute this toxic substance.[1] Alteration or decomposition of organic material (e.g., the albumens, etc.) is known to result in the formation of various substances like the ptomaines, tyrosine, creatine, etc., some of which are toxic. Again, the chemical interaction and its results in one case may differ slightly, but all-sufficiently, from those in another, for instance in the case of an epithelial cell as compared with a nerve cell; just as the toxin produced by one bacterium differs in some remarkable manner from that formed by another. Where the question is one of organic processes by different living cells, both specificity and diversity can be readily understood. On the other hand, the very fact of this unusual ingestion and attempted digestion itself, with its various implications and possible accompaniments (e.g., multiplication, inflammation, necrosis) may suffice to explain the symptoms in certain cases.

Now, what is to be regarded as the virus, the ætiological agent which starts hæmatophagy and the disease? I suggest that this is some *ferment*, the ferment produced to attempt the digestion, by the particular type (or types) of cell concerned; this ferment will vary, of course, in different

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[1] It is important to note that, certainly in one case, these formations are *not* toxic; i.e., the result of unsuccessful digestion is not injurious. I refer, of course, to the Kurloff-bodies in the guinea-pig's lymphocytes. Now, as indicated earlier, the contained elements here are morphologically different from the "inner formations" in, for instance, the Negri-bodies; they tend very frequently to be in the form of curved rods and wavy threads. And in the case of the corresponding bodies found in syphilitic lesions, and regarded by some authors as associated with *Treponema pallidum*, these, too, are very similar to the Kurloff-bodies, often showing wavy threads, etc.; here, also, they appear to be, at any rate frequently, in lymphocytes. Hence they, too, may not have any ætiological connexion with the disease (syphilis), although they probably are an abnormal consequence thereof. What all these varied proteid substances are will be a difficult question, I am afraid, even for the bio-chemists to answer.

diseases. I would account for the necessary increase in amount of the virus and for its spread and dissemination in the following way. Infection with a minute quantity of the ferment *stimulates fresh cells of the particular type to hæmatophagy and the production of more of this same substance.* Granted the stimulant property, the metabolic processes in connexion with hæmatophagy will supply the "multiplicative" factor. Thus inoculation will lead, after an incubation-period, to the development of the visible signs and symptoms of the disease. More and more of the virus will be produced, until, in a favourable case, antibodies developed in response are sufficient to counteract it.

That this is no merely fanciful view is indicated, I think, by what is known in connexion with the so-called "bactériophage" of d'Herelle, which provides a most instructive analogy. The consensus of best-informed opinion is, I gather, that we have here no actual living micro-organism, but a bacteriolytic ferment ("catalyser"), a minute quantity of which induces the bacteria themselves to produce more of this lytic agent and, as a consequence, to undergo autolysis. Essentially, the process may be regarded as one of *self-digestion.* In the case of the virus of the diseases under consideration, the main difference would be that the ferment induces not self-digestion, but (attempted) *blood-digestion*, at any rate primarily.

Consider, in this connexion, the closely allied cases of vaccinia and variola. The upholders of the parasitic nature of Guarnieri's bodies have maintained that one principal cause of the difference between the two diseases is due to the fact that in the former case the parasites remain intracytoplasmic, and in the latter they become intranuclear as well. We have to deal, however, not with parasites, but with ingested blood-elements in process of alteration. And I can easily understand that, in response to a strong stimulus, the ingested material may become actually included within the nucleus. If readers will turn back to figs. 7 and 8, it will be seen how, in the course of normal hæmatophagy by the megakaryocytes, the ingested food may be completely surrounded by the nucleus; actual penetration is only a stage further (cf., indeed, fig. 4*b*, where corpuscular fragments are actually inside the nucleus of a large mononuclear). And where the alteration takes place within the nucleus, under abnormal conditions, the substances produced, including the ferment, may well be more concentrated and potent.

The dissemination of this ferment-virus will be brought about by the breaking down of the digesting cells; indeed, it may be discharged even before this takes place. I have above indicated, in the case of the Negri-bodies, that this ferment can probably be poured out of the nerve-cells. Its transmission by means of dejecta, etc., can be readily effected.

As regards typhus in this connexion, in a recent review by Banus (*Intern. Journ. Public Health*, 1, November, 1920, p. 351), reference is made to certain work by Kusama, which may be, I think, of the greatest significance. This author claims that the virus in the blood is attached to the

blood-platelets; he isolates the virus from the platelets by grinding them with the plasma, in which the virus is then found after centrifugalization. Even then the virus does not pass through the filters. *This is precisely the situation (in the blood) in which one would expect the virus to be most concentrated, on the view here put forward.* I mentioned above that although, as cells of endothelial type are concerned, the digestion might be expected to be fairly complete, with production of platelet-cytoplasm; nevertheless, it would probably not be completely normal, as these particular cells are not those which customarily exercise the function of macrophages (they are specialized to form the walls of capillaries, etc.). Therefore, in this metabolism, both an unusual ferment and (perhaps) slightly different by-products will be concerned. This ferment, the virus, will be *in the platelet-like bodies resulting from the platelet-cytoplasm produced by these endothelial cells in the capillary lesions*, but it will probably not be in the normal platelets, normally formed by the customary macrophages.

Many will still hold that there is some invisible microbe, either inside the platelet or fortuitously attached to it; others will say that what are considered to be platelet-granules are really Rickettsias; but I think it is most likely that the bodies regarded as *Rickettsia* are in reality, elements essentially comparable in origin with platelet grains and granules, and that the platelet-cytoplasm and platelet-like bodies containing them, differ chemically in some most important respect, which constitutes the active principle of the virus.

There is a saying that one is sometimes in danger of missing the wood for the trees. On the other hand, it may be said that I have not paid enough attention to the trees, because of the wood. Up to the present, I have only been able to study three in any detail, and there are probably many trees in this particular wood; to know them all completely will mean much further work, by many workers. *But there is a wood*—a fascinating wood—and as soon as I realized this I became most desirous of gaining some idea of it as a whole, its general character and apparent extent. Viewing my wood from afar, I believe that I have caught a glimpse, indistinct though this be, of its nature and configuration. And from this distance, it seems to me there stands within its confines a giant of the forest, towering above the other trees and spreading wide its gnarled and twisted branches; a thing indeed of menacing aspect. This gloomy, malign form, I am now endeavouring to approach more nearly, intent on learning whether, of a truth, it also is included in the wood called hæmatophagy.

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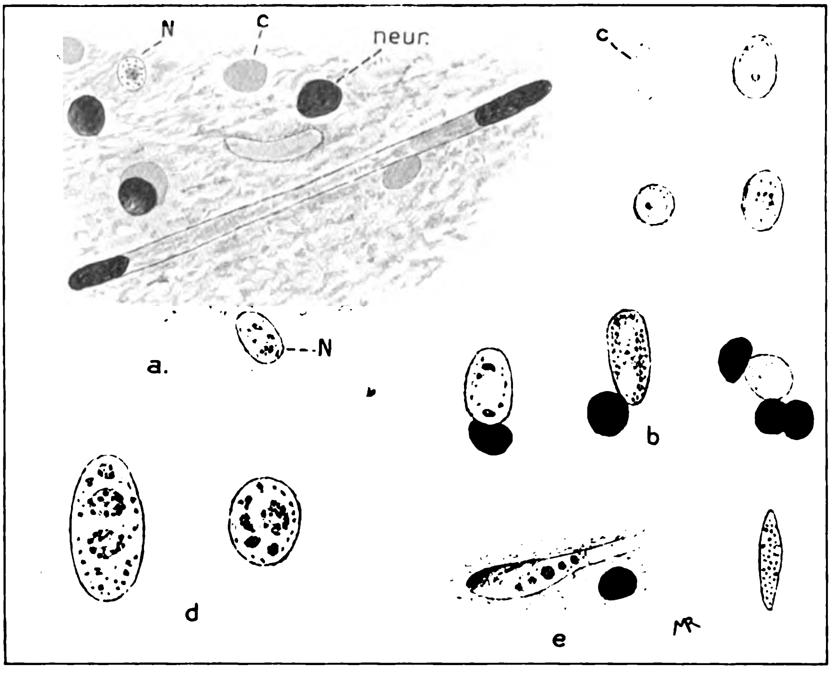

FIG 13.

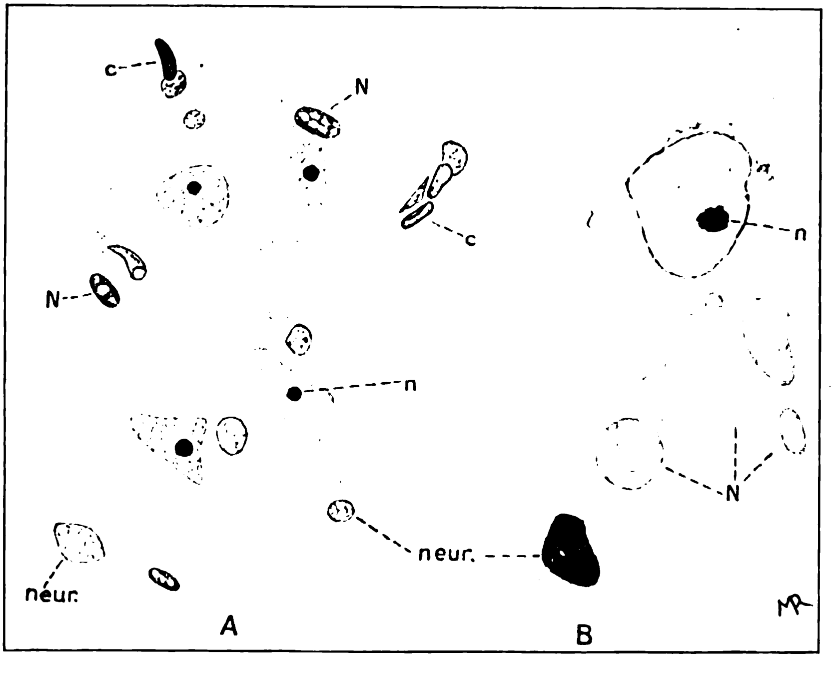

FIG. 14.

To illustrate "An Introduction to the Study of Hæmatophagy," by H. M. WOODCOCK, D.Sc.Lond.

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EXPLANATION OF FIGURES.

FIG. 13.—Stages in the transformation of corpuscles into Negri bodies. (*a*) Portion of a field; this shows a long capillary with an endothelial nucleus near each end, and containing (close to the right-hand nucleus) two corpuscles in contact; other unaltered corpuscles (*c*) are scattered about, also neuroglial nuclei (*neur.*); a short, rather curved capillary segment, rounded and closed at each end, in which the contained corpuscles are polychromatophilic; two Negri bodies (*N*), in an early stage, are also seen. In the right-hand half of the figure are various stages in the alteration of corpuscles or corpuscular masses; (*b*) a portion of a corpuscle still quite unaltered is contained in this mass undergoing alteration (at the lower left-hand corner); (*d*) fully-formed Negri bodies; (*e*) a narrow capillary segment containing a fully developed, cigar-shaped Negri body and one or two unaltered corpuscles. (Smear, Giemsa; all × 800 excepting (*d*), × 1,600.)

FIG. 14.—Negri bodies in sections. A, Field from a section stained by Mann's method; B, small portion of a field stained by iron-hæmatox. + eosin; (*c*) unaltered corpuscles, often crinkled; (*N*) Negri bodies, or altered corpuscles becoming such (in B); (*n*) nucleoli of nerve-cell nuclei; (*neur.*) neuroglial nuclei. (A × 800; B × 1,600.)

FRAGMENTS.

By Colonel Sir ROBERT FIRTH, K.B.E., C.B.

XXIII.

The lengthening days of late January suggest how alert is the mind in the early weeks of the year to notice and welcome the faintest sign of winter yielding a few minutes of its gloom. It is puzzling how we notice the little change without measuring each day or consulting clock and almanac; it is an evidence of how the subconscious part of the mind may keep a more accurate register of delicate changes in Nature than many credit it with. The early nights of this season are curiously beautiful, especially if moonless: two stars in the south force themselves on our notice, they are Rigel and Betelgeux, tremendous suns which light the feet and head of Orion. They pulse and quiver, giving a real pleasure to anyone watching their restless play through the branches and twigs of trees. Adding to the beauties of the hour is that Amazon of the skies, Eridanus, streaming west with startling brilliancy whilst, just behind, comes the Dog Star outshining all in rare distinction. A stroll through the woods in ten days' time will reveal other portents of the coming spring, because the tracery of the twigs in the elms seems to thicken in February owing to their being flowered richly with wine-red blossoms. In the meadows, the willows will be pearled with grey-white palms having a distinct blush of pink.

If luck be with the observer and he finds himself later on in early March among the pines and birches, he will see the pines bitten out, hard and clean, against the sky whilst close by, in contrast with their purple-blue, will be the white-trunked birches with twigs of pale claret. Truly, a glorious vision of contrast and harmony, exampling one of Nature's boldest efforts in effective and simple landscape gardening. At such a time and in such a place, let him look for the whitlow-grass and moschatel, two tiny plants which are true diminutives, and curiously different in their habits. A sun-lover, the whitlow-grass is to be found in these early days on sandy heath and in most meadows as a tiny plant presenting a dainty rosette of green leaves from the centre of which springs two inches of stalk cropped with hair-fine pedicles carrying speckled white flowers. So delicate are the supporting pedicles that it seems as if a forcible breath would annihilate the whole, and yet associated with each flower is a tiny pod crammed with dust-like seeds. In contrast with this harbinger of spring is another tiny plant which flowers early, and that is the moschatel, which seems to avoid the sun and thrives best on the cold side of a bank or hedge. It is curious, but perhaps love of moisture makes it shun the brightness which, ordinarily, is the main factor in the making and ripening of seed. The result is that moschatel is a consistent

perennial and, unlike the whitlow-grass, independent of a cold spring or summer and bad seed season.

Certainly, many days in March are cruel, yet in almost any spot off wind-swept downs there is abundant evidence of spring; the hedgerows are full of buds, the ditches and hedge bottoms have plenty of new leaves of chervil and other umbelliferous plants, with goosegrass or cleavers already making ready to scramble over every neighbour. Flocks of chaffinches whirr up from the fields, suggesting winter still, but their slate-blue heads and gaudy breasts belie the suggestion and announce clearly that it is spring. Even the song of the missel-thrush argues both for winter and spring, but even over his head where he is singing the peewit tumbling in the air gives the casting vote in favour of the fact that winter is in full retreat. It is the same along the coast, for the season is quickening in the salt dykes and pools of the estuary. There, some of the oziers have fire-red in their twigs and others are tipped with purple or faint lilac. Even those deadest trees of winter scenery, the alders, are alive again. True, their thick drooping tassels look as if they had been scorched by frost and perished in a premature attempt to flower, but so soon as we pick a few there is no longer doubt as to their being in their pollen prime.

In these days of March, the observant eye will note other facts such as the energetic but rash honey-bee circling and flying far afield from the hive. She may have a map of the flower world in her brain, but evidently she has no almanac of the seasons. Tempted by a balmy day, she leaves the cluster of her fellows and its associated warmth to scour the country for honey and pollen which do not exist. The penalty is death. It is difficult to know why she does it; it can hardly be for exercise or pleasure, possibly an impelling force to seek fresh stores wherewith to fill her golden larder now becoming empty. Should she survive her quest and return exhausted and cold, no sympathetic welcome awaits her as a reward; she will be recognized by those on guard as a profitless or damaged worker, and sacrificed ruthlessly. Spartan rule dominates the community of bees, and sympathy or aid for a companion in pain or distress is unknown. As April opens out, other details in insect life become apparent, not the least interesting being the house-hunting of an early awakened queen-mother of the common wasp. She alone has outlived the wreck of last year's city, located, perhaps, somewhere in the very bank along which she may now be seen searching diligently for a site in which to found her new community. Carefully she explores each hole and hollow, often disappearing down one or other of them for two or three minutes, only to come out again and be off on her quest again once more. At last a choice is made and then the necessary excavations are commenced wherein to shelter her coming brood. Few human house-hunters can be more fastidious than this yellow queen.

The birds at this season have a charm all their own. In no bird is the

strong wine of the spring more evident than in the blackbird. He is volatile and sprightly; he has his flute to sound his joy and a golden dagger with which to stab and scatter dead leaves in search of grubs. When on the wing he chatters, then perches, looks and pries into things, goes off again and perches once more. His very way of perching is all his own, up goes the tail slightly open like a fan, then down and up again, he opens and shuts his wings with little flips some half dozen times, when off he rushes chattering as though annoyed to know that, besides himself, there are other creatures prepared to get some pleasure out of this best of worlds. To hear him singing to his mate is equalled only by the corresponding lay of the thrush, and the song of either is excelled only by that of the nightingale and lark. The peewit is said to be the most wakeful of our birds, but I think the thrush runs him close. Many an evening in early summer have I heard him singing at 9 o'clock, and yet five or six hours later the same bird will be in full song again. At such an hour the thrush's song may be less brilliant than that of the nightingale or lark and less delicate than that of the blackcap and garden warbler, but it is long, strong and faultless in phrasing. The music of a thrush at his best is delightful and the song of a thrush at midsummer is far superior to that of one in January. I fancy the thrush knows that, for he practises and repeats, evidently dwelling admiringly on certain passages in his song. This suggests that the thrush has a refined ear for music and can appreciate differences between the crude and perfect, much as we do. It is different with the whistle of the curlew, the wail of the peewit, the drum of the snipe, the moan of doves, the croak of crows, the whinny of waterfowl or the mew and scream of gulls. None of these have a real beauty of sound in themselves; they depend on their environment and on our mood, and yet, to the musicians themselves, the music cannot sound inharmonious. If this is so, then the thrush is nearer to man in his ear for music than he is to peewits, snipe, rooks, crows or waterfowl. Who knows? The peculiar features of some birds' language are as interesting as are the features common to the language of various birds. Why should the cirl-bunting and the lesser whitethroat warble and not the common whitethroat, goldfinch and yellowhammer? The warble of the cirl-bunting and the lesser whitethroat is distinct, a warble low, gurgling and carrying no distance, but prefaced by a bubbling note or shake which can be heard a hundred yards away. No other warbler has this note and yet, why do two such birds as the cirl-bunting and lesser whitethroat, utterly unlike in all other ways, share this tremulo or bubbling shake so out of proportion to their size and tiny throats? It is one of those facts in Nature which it is more pleasant to hear and know than it is easy to understand.

The song of another bird which arrests attention in late winter is that of the missel-thrush. He is the big, grey relative of the common thrush of our gardens. A real countryman and the Peter Pan of birds; the bird of eternal youth and vigour who never grows up is often known locally as

the stormcock because he delights to sing upon a breezy day. His favourite place is on the topmost twig of a tree, whence his optimistic spirit overleaps the hard times yet to come and makes him send a message over the parish. There is no great range of variety in his song, for he repeats the same phrase again and again, but no common or song thrush ever approaches the clear and powerful notes with which he challenges a coming gale from the west and proclaims the spring. He is a bold bird and one who stands no nonsense from feathered intruders who wish to share some berried holly or a tree of crimson haws. At the breeding season, missel-thrushes display some recklessness for they build early, long before the leaves appear, and scorn concealment. They are very fond of building among or near branches of mistletoe, hence their name, and the parent birds will attack furiously any jay, magpie or hawk who ventures to approach the nest. I mentioned the peewit or lapwing just now. Many think them to be uninteresting birds. Far from it. If anyone doubt, let him watch a pair over the young corn which hides their young. If not flying round, just overhead, the hen will be taking little runs broken only by digs for food and a wifely call to her mate, who, circling and curving in the air, keeps up constant calls of encouragement. In these circlings, the bird turns sideways on alternate wing, showing striking contrasts of white and dark plumage according as to whether the under or the upper side is exposed. These exercises in the air appear to be non-utilitarian unless they be a necessary part of the courting business. This tempts me to say that we need more than a mere theory of utility to account for either the song, bright plumage or love-antics of birds because, in many species, courtships and gallantry continue long after mating, and the establishment of the nest or home. Watch a pair of wagtails and you will see the cock feeding the hen long after the eggs are hatched. It is the same with many other birds and clearly a hint to humans that the amenities and practices of courtship are intended by Nature to be continued into the later stages of domestic life. Of the more common birds, who can fail to love the hedge-sparrow and linnet? Not a showy fellow, the hedge-sparrow ever reminds me of a well-dressed man whose clothes are of a quiet pattern, in the best of taste, well cut and perfect fit. It is curious to note how shapely and elegant are all the birds clad in dun, brown or drab, or other quiet hues. The linnet, red-breasted and erect, sipping the dew from a green leaf on a summer morning, is a pleasing sight, but the bird is chiefly interesting on account of his rôle as a producer of its kind. Next to the common sparrow, he is a most enterprising parent, for no sooner is one brood fledged and flown than another nest must be begun. A season of three nests is their average, and during all that time the cock is in song, which has been aptly called "a pretty affable little song and not more perhaps."

Then there is that delightful bird, the chaffinch. When spring is in the swelled bud, then the chaffinch breaks into his song which is really

one of the pairing season only and leading on to the lichened nest which most of us know so well. The thought of such a nest carries me back to boyhood and memories associated with that very choice thing, a stonechat's nest, that rounded cup of fine grass with moss woven in and a feather or two for lining. Very difficult to find they are, for a pair of stonechats can baffle a bird-nester for weeks. Once found, the beauty of the four or five blue eggs, often faintly freckled with brown, makes it a sacrilege to touch. The gold of the gorse bloom in whose bushes they are often to be found and the delicate shade of the eggs make a picture hard to beat. Other nests, of which I have a lively recollection, are that perfect architectural effort peculiar to the wren, also the crazy jumble of straw and grass crammed with feathers and filled with five pretty white eggs all speckled with red of the careless chiffchaff, and the solid built cup of fine hairs made by the goldfinch, a cup so small that by the time the sharp-eyed young birds are nearly ready to fly there is barely room for them, with the result that many tumble out before they are fit to fly. Thereto attaches an important question and many a tragedy. No one knows what determines the right moment when young birds should leave their nest. Possibly the parents do not know and yet they are the infallible judges of the arrival of what is the most critical period in bird life. Premature departure from the nest means certain death, for the coppice or hedgerow is a jungle full of preying beasts, to say nothing of weather, too hot, too cold, too damp, too dry. Nature's tooth and claw are red where young birds are concerned; she seems to take no pains to preserve each chick or birdlet that she brings into the world. I know of no more moving sight than the finding of a dead young bird in wood or hedgerow. It is difficult to be unwrung or avoid the questioning thought, what that bird might have been and what it is. I recall this passed dry summer, when young birds were lying about the countryside, and I was tempted to say, "it out-Herods Herod, for this is the slaughter of innocents." Once more, another of Nature's mysteries.

With regret I leave the birds, and would look at other things. On a warm afternoon, in late April or early May, when the anthem of willow wrens is at its height, there is no spot to beat a coppice I know between Amwell and Easeneye in Hertfordshire overlooking a cress farm watered by the Lea. Its carpet is the dead leaf of beech and wych-elm studded here and there with anemone and primrose on a groundwork of green given by the leaves of the wild hyacinth and all shimmering with the blue of its flower. Verily, some goddess of flowers must have walked through that coppice to leave it so blue. A meadow near by is speckled with the yellow of cowslips, while the adjacent beech and oak woods have that delicate bloom that comes before the full green. This bloom is characteristic of the season, having no splendour but rather delicacy and fineness, through whose blur the dark strong limbs of the trees are seen in all their boldness and varied shape or curve. The whole scheme of meadow, copse, and

marsh is as painted in water-colour, whereas the day of later summer is done in solid oils. Apple-blossoms, the green of birch, and the white and red cattle in the fields, all give the idea of lightness before all else. To sit idling there in such a spot is an education. Watch an individual plant; it seems to grow as one regards, the living rivers of sap are in full flow, drawn in, pumped up, finding a way everywhere up through the root of the tiniest grass and through the bulkiest forest tree. The woods, fields, and hedges are in a seething, bubbling state of change and production, driving, diverting, and working up the elixir of early summer into nourishing food, serving it out for the use of plants to-day or cunningly storing it for the needs of to-morrow. In truth, a laboratory, and yet outwardly there is the unruffled calm and ease of May with no signs of the maze of machinery actually in full motion. Look, then, at some plant or flower—one of the many finished products of it all—and watch a wild vetchling of the pea family and a bumble-bee which, alighting daintily on the wing petals, probes for nectar. Apparently no service is rendered to the flower in return for its sweets, and yet there is, because the insect is dusted with the pollen whilst she helps herself to the sweets, and this pollen she carries off and gives to some other flower. And yet another wild blossom arrests attention; it is the red campion in all its glory of shades of pink. It is a plant with some individuality, and in perennial competition with the hyacinth. They grow collectively near each other, but individually the plants do not mingle. The reason seems to be a matter of light and shade. When the underwood shoots grow high and shut out air and sun, the hyacinth takes possession since it flowers and seeds almost as freely in shade as in sun. On the other hand, the campion can flourish only in the open, and it is not till the undergrowth is cut away that its long buried seeds can germinate and renew the struggle with its competitor the hyacinth and every other spring-flowering plant. Not a fern or blade of wood grass shows among a patch of campion. The foxglove alone will not be denied, and it alone competes indifferently with success—though later on, when campion has ceased flowering, the meadowsweet, the pepperwort, and lilies of the valley will assert themselves.

Towards midsummer, for weight of scent and bulk of bloom the hawthorn stands first of wild English flowers. It is at its zenith with the year at zenith, and with its constant companion, the chervil, makes the great white week of the year. But for quaintness about this time, commend me to the hornbeam. Leaf and blossom at each twig tip seem like some curious insect, but, on looking closer, it is seen that the male flowers of the hornbeam resemble caterpillars, while the smaller female blossoms, with their splashes of red, that sit beneath to catch the pollen, are still more like caterpillars. And what about the mystic markings on wicked hemlock and on innocent chervil leaves which are to be seen abundantly in glorious June. Are they mere warnings to cattle to be aware and touch not—and yet why should harmless chervil and its follower chærophyllum impersonate

the hemlock? Tis all but one more mystery and one of the many marvellous things to be seen in June—that month which always seems so short and yet so full of days when, with the dog rose in full bloom, it seems as if "the ball of the earth were delicately poised on the finest point of time." Then, and in the succeeding month, it is pleasant to lie at ease on the virgin turf of some chalk down and realize how the great bouquets of the elder stand for wholesomeness, the pink and white dog-roses for innocence, and the wild thyme for all that is aromatic. And yet on such a down, with face to breeze, the truth comes home that the bird's-foot trefoil, too, has a fragrance, faint and elusive, but chalk-down sweet. The wild grasses of such a time are dusty with pollen, and their seeds defy the best of clothes brushes. Many overlook the grasses in their flower and seeding season, but, on close scrutiny, they repay attention. The rough cock's foot, with its weighty flower and seed-head bending over for want of stiffer support, the tuft of timothy, the tuft of foxtail, and the tuft of bent-grass are all partners in the apparent waste, dividing its acres, towering high in their season, but all marvellously beautiful, and yet distinctly apart and each to itself. Buried often amid the jungle of their stems can be seen the tiny mill-mountain's fairy bell, so fine and delicate that any butterfly but a Bedford blue, which sits upon it, would weigh it down. In spite of this, there still be some who have lingering doubt that our world was made by chance. Then in such a place, and on such a day, there comes a sound across the down which tells the presence of the bird whom Matthew Arnold called "the too quick despairer." It is the cuckoo who, like the swift, is quick to wind up his visit to our shores, and reminds us that the summer will cease to shout a few days after he has ceased to call.

Soon after midsummer, a curious silence seems to fall on the birds; midwinter in southern England is far more vocal. Nearly all birds have finished nesting and are now plunged in the dejection of their moult. Even so, the bird voices are still to be heard by him who chooses to listen, but they are the voices of the anxious parent rather than those of the ardent lover. Yellowhammers still keep up their plaintive chanting by the hot highways and goldfinches their livelier sibilant chatter in the leaves of the garden shade-trees. Both these gold-plumaged birds have often a very late second brood and carry their spring song far into the dog-days. Even at the waterside, the silence of the woodland is reflected in the faded beauty of the mallard drake. A few weeks ago, his glossy bright green head, snow-white collar, auburn chest and silver-grey sides framed in black and white made harmonious contrasts, finished off fitly by the up-curled sauciness of his tapering black tail flaunting a challenge to produce a handsomer bird. But, to-day, all his gay attire has departed, and as a dull brown bird he seems even less decorative than his drab wife. His habits have changed too. No longer, like a gentleman at large does he loiter on the surface of the water that mirrored his beauty, but now he haunts the quiet waterways between the rushes and by an anxious "querk,

querk" proclaims his presence as one of two solicitous nurses of a large family of clumsy ducklings. With the silence and the loss of beauty which comes to bird-life at midsummer, there is, however, a consolation; for they mean only that the music and colour of next year are in the making, that after the flower comes the fruit and from the seed will come the flower again. It is the same if we look in the hedge-rows; it will be six or eight weeks before the hazel-nuts are ripe, yet in the axils of the leaves, still green, we see already the rudimentary catkins which are to fertilize next year's nut crop.

Yes, there will be signs all around at the end of July that Nature is quieting down. The evening air will taste of new hay, and the meadow of that day is a different world from the meadow of June. The ghost and the swift moths will still be flying, perhaps in lessened numbers, and the night-hawk will come from the wood and silently sweep just over the surface of the grass. True, the evenings are still long, but the sun will set soon after nine. The glory of things is toning down, the hawthorn is green again and the gorse looking shabby, but there still are good things to see and note. The redstart will be chattering in the evening quiet and the nightjar very much on the wing in search of late moths and chaffers. In the early August days we begin to miss the screaming wavy flight of the swift over the housetops and other summer birds, whose young are already strong on the wing, are forsaking their breeding haunts to wander southwards. Thus, I look with confidence for grey wagtails and common sandpipers by the river and wheatears and whinchats in even the towniest parks, with strange hawks overhead. Other signs of the times are the increase in the flocks of roosting starlings and the banding of peewit families into large winter parties, coupled with the fact that the first black-headed gulls are returning to the Thames, though still in breeding finery. The night sky is interesting and suggestive because in the north-east the Pleiades are coming into view, this group being the precursor of the winter stars. Venus is a morning star rising about an hour after midnight. She is the only visible planet, since Mercury rises and sets too near the sun to be well seen, Mars is above the horizon for only an hour before sunrise and scarcely to be seen in the dawn, whilst also both Jupiter and Saturn set in the evening twilight too low down to be conspicuous.

Though the shortening of the eve daily may become more marked, departing summer will not yet have removed all its extravagances. The flowers may be less blatant but the insect world will still be much in evidence, especially in respect of butterflies. The peacock butterfly will still be seen in the lanes and the red admiral in the gardens; it is curious why Nature brings to life in late summer and early autumn such a host of these beautiful insects. Not the least interesting are the fat caterpillars of the elephant hawk-moth which are to be found feeding in August pastures on the leaves of the greater willow-herb. Their great rolling eye-

balls are a remarkable example of protection by intimidation. These apparent eyeballs are merely a pair of spots on a segment near the head. As the caterpillar rests hunched up, the segment bearing these ocelli is enlarged and thrust forward, giving a grotesque and savage effect. Doubtless this basilisk glare saves many of these caterpillars from bird attacks. Several other species of hawk-moth caterpillar to be found at this season are protected by patterns which conceal them; lime, poplar and the eyed hawk-moth larvæ all have streaked patterns which blend with the veining of leaves and often a horn on the tail which mimics the stem of a dead leaf when they seek the ground. But the weirdest of all is the pattern of the larva of the elephant hawk-moth, whose glare is its salvation. The cloud effects at this season are remarkable, for the north and north-west horizon in the short hour of the afterglow is often turned into a wonderful cloud factory. All the vapours in the air seem sucked down into the region of the setting sun, there to be worked up into streaks and stripes of cirro-stratus cloud. Some look as if they were the work of a paint-brush, and others suggest the tools of a master-engraver in line work, stipple or mezzotint. It is the same with the landscapes; the green is going fast, but the second bloom of the clematis and honeysuckle make amends for signs of decay in coppice or hedge-row. But the foliage of the distance is not discoloured like that of the fields and hedges close at hand. The gold in these late August landscapes shows off the purple, the purple enhances the gold, and the chequer-work of corn-gold or pale straw colour worked in with blue hill and purple wood make it difficult for any one with any artistic sense to doubt that the barometer of life is still high and set fair. The only things the attentive observer will miss are the birds; where are they at such a time? Save for sparrows, redbreasts, thrushes, blackbirds, chaffinches, starlings, rooks and ringdoves, the familiar inland lanes and coppices are almost deserted by bird life. To find old summer friends, we must go to the slopes and steeps by the sea. There, there will be found the constant chirp and flitter of small birds. Under the air-light sand martins are restless parties of linnets, with a few goldfinches to keep them company. Yellow wagtails are there too, and in the brambles hide chiffchaffs, whitethroats, willow-wrens and whinchats. All these are would-be travellers, and one wonders how many will reach Africa or wherever it is they make their winter home.

At dawn the redbreast, at dusk the rook; these are special sounds of September, October and late autumn when the cornfields are cleared, hedges trimmed, traveller's joy going to seed, knapweed almost over and the morning and evening air endowed with a suggestive sharpness. At seven in the evening there is not much more light in the sky than there was at ten in June. A stray note may come from the rook trees and save from the tawny owl who has begun again his trumpetings of autumn and winter nights, there will not be a sound from any other bird. A few hours later, Arcturus will have set and all the Pleiades be once more well

up on the eastern sky. The only colour scheme in November is to be found in the woods. The beeches turn from umber to copper or fire-red, and their leaves lie uncrumpled without a speck on their bold clean yellow. The leaves of the sycamore lie spattered with black and those of the aspen a gloomy grey. The oaks give a variety of tint, pink, orange, purple, umber and ochre, but these entrancing tricks of colour are not to be seen by an eye that pries too near and curious. Occasionally on days at this season the trunks of ash and oak stand out with a strength and freshness, not seen for months, which suggests and emphasizes the fact that the natural year is over and that now, when the wood is stripped, the tangle of summer undergrowth, the miscellany of spinney and hedge-row beaten to the ground, the natural year begins anew. In that thought I feel content to close my almanac from Nature, coupled with a parting suggestion to the reader that, until January's lengthening days warrant other observations, he may not fail to get some pleasure by the occasional contemplation of a midwinter night sky, if free from cloud. On such a night the seven equal stars of the Plough are glittering low in the north, just clear of the horizon, with the pointer directing straight up at the Pole Star; and Alcor, a tiny star but yet a mighty sun, close to the mid-star in the plough handle. Its close neighbour the Lynx will hardly be seen, but, north-west blue Vega blinks intensely, marking the loveliest of the never-setting stars of our latitudes. Cassiopœia will be seen on the zenith with Cygnus near by; while, down low in the west, Altair, the chief star of the Eagle, still shines bright. To the south-east Jupiter with Orion in his train will stand clearly out to view, but not so Venus, for, at this season in the smouldering ruin of the day, her torch becomes extinguished and the planet of love and flowers is gone; once more it is winter, and the aloof spirit of slumbering Nature broods on the earth.

XXIV.

How wide is the gap, and still how close the bond between the simple and the intellectual, was borne forcibly upon me by a recent meeting with my old friend, Uncle Remus. I found him but little changed, as wily as Brer Fox, and as simple as Brer Rabbit. We had not met for many years, and yet his humour, interpretations and superstitions had all the old charm of the primitive world, as when he said: "Come down to dat, Brer Ab, dere ain't nuffin 'dat ain't cu'us." How true! At the head as well as at the foot of human nature the impulse of curiosity is dominant. He sounded the note of all philosophy, the note that made Aristotle a deep observer of the seeming little things in order to understand some of the apparent great ones. Here the learned descends to the level of the unlearned, and the philosopher is no nearer to Nature than the negro in all his ignorance and seeming blindness. Uncle Remus and his kinsmen are close observers of natural things, instinct taking the place of reason. In

their hands, instinct grapples with mystery, and artlessness fills the place of art, simplicity of mind becomes identified with the mysteries of natural things, and the immanence of the instinctive mood renders ingenuity impossible and reason superfluous. In the stories of the negro, as developed from Kaffir folk-lore and told by Uncle Remus, humour is untrammelled by learning and quite unfettered by ordinary rules of ethics, whereas in the hands of the more sophisticated humour invariably is tainted with criticism, or becomes strained and cynical. The reason of it all is that the primitive people of Nature's world hear the right sound and sense the right mood; there is no artificiality, the interpretation being simple, naive, direct, agreeing with the habits and cunning of the animals of the same world; for the negro, left to himself, is most natural and most unself-conscious, living with his fingers on the pulse of Nature.

As among many aboriginal people, such as the Arunta of the deserts of Central Australia, and the Ba-Ila speaking races of Northern Rhodesia, there is plenty of superstition in Uncle Remus, but it is harmless; even were it not, it may be affirmed with truth that much superstition at the illiterate bottom is less dangerous than a little ambition at the intellectual top. In the realm of mystery, the primitive man is the equal of Hamlet in the grave scene, and no wit worse off than Macbeth in the presence of the witches. Uncle Remus says he "despises fer ter heah dogs a-howlin' an' squinch owls havin' de agu, out in de woods," and that these things make his "bones cold an' his flesh creep." Even Lady Macbeth, when she halts on the doorstep and listens to the owl's scream, could not say more. Those are the times when the phantasies born of the darkness loom big, stars and planets give place to portents of the earth, every sound becomes prophetic, and their echoes strike chords in the imagination on the whole gamut of mysteries, secrets, delusions and illusions.

It is in this realm of mind, disturbed by emotion, that the gap between the simple and the intellectual perhaps appears the widest. Primitive man, or the negro and the butcher's boy, can talk of a secret with some degree of intelligence, but they cannot discuss intelligently a mystery. To each of the three, and to the intellectual man, a secret has little interest once it is understood, but the latter knows that while secrets are finite sensations, mysteries are infinite realities because the more you approach a mystery the farther it recedes into higher regions. Newton revealed a secret of Nature and made it plain, but neither he nor Einstein after him solved the mystery behind the secret. To the ignorant and superstitious, everything tricks and deludes. Delusions are fixed deceptions, but illusions are related to time, health, social circumstance and intellectual mood. While to the simple and uncultured everything deludes, to the intellectual, everything is illusive in the sense that realities lie hidden under the shifting lights and shadows of sight and sentiment, and only those who have the key of knowledge can sift the wheat from the chaff. Though the primitive and simple-minded have many delusions, they are astonishingly

free from illusions; the reason would seem to be because they are as a rule so close to Nature. The woods and the fields are among the few places where the mind is serene. The woods, hills, valleys, plains, streams and stars are impersonal. A flower does not disappoint us when it begins to wither and fade, nor does a tree when its leaves begin to fall. We apprehend them as realities that change their appearance at fixed periods, and their mementoes are connecting links that stretch from month to month, and from year to year. It is, however, when we enter the civilized and artificial world of man that illusions seem threatening and afflicting. There, and there only, do things and people worry and deceive; and yet, in spite of it all, it is through the stormy straits of illusion that most of us reach the calm waters of the harbour of serenity. And when disillusion comes aboard our barque, while it may give us a momentary shock, it ends by producing a settled satisfaction unknown before, a satisfaction which the coveted prize so long held before us in our visions and dreams, could it have been attained, would hardly have secured. It is not the will that acts, but life itself; and the lesson of it all is, that happiness depends on being able to sift the things that glimmer and deceive from the things that are simple, real and fixed. To this faculty of clear vision to see things as they really are, to be simple and natural, do Uncle Remus and others like him owe their advantage in respect of many matters over us.

Apart from such considerations, the stories of Uncle Remus, so ably put together by Joel Chandler Harris, have an ethnological interest. By most people they are regarded mainly as children's stories, and very good stories they are for children; but they are more than that. Though actually the legends and myths current some sixty years ago among the negroes on the plantations in Georgia, Carolina and Alabama, they nearly all have their variants and counterparts in Kaffir folk-lore, and in the tales current among the Indians on the Amazon, who doubtless obtained them from African immigrants or imported slaves. In the story of "The Jabuti that cheated the Man" to be found in Hartt's "Amazonian Tortoise Myths," the Jabuti is identical with Brother Terrapin. In like manner, the Brother Rabbit of Uncle Remus and his negroes is the hare given in the story of Hlakanyana in kaffir folk-lore. The negro story of how the Bear nursed the Alligators is but a variant of Hlakanyana and the Leopardess, while the Kaffir story of "The Great Chief of the Animals," is nearly identical with the negro story of "The fate of Mr. Jack Sparrow," and Uncle Remus' story of "Wattle Weasel" is very like the Kaffir story of the hare. The negro story of how Brer Rabbit made a riding horse of Brer Fox is also current among the Amazonian Indians, and the same can be said of the negro coast story, "While the Alligator's back is Rough." It is also worthy of note that a terrapin or tortoise, which stands next to Brer Rabbit among the heroes of Uncle Remus, is the cause of the death of Hlakanyana, or the hare, in Kaffir legend. Many other examples or

cross references might be given, but they all indicate that the real origin of the relatively modern negro tales, as given by Uncle Remus, goes back many centuries to the legends and myths of Kaffirs and Hottentots, and that their cradle was Africa, and that the link which connects them with America is the horror of the old slave trade and the tragedies and sufferings of the "middle passage," as the journey across the Atlantic was called by Creighton.

The quaint stories of Uncle Remus may not have a fascination for all, and my mind may be a somewhat old-fashioned mind, but I own that these relics of the past, or survivals among fore-lore, have a present value to me, because I feel that to ignore their living function is to lose touch with that movement of history which the folk-lorist has the chance of studying at his very door. For the life of the folk, being rooted in Nature, would seem to be more fit to endure than any form of the cultivated life which springs out of it and, in time growing tired as it were, reverts to it again. The continuous life of the folk constitutes the germ plasm of society, and even if, as we must all hope, the life of man be no mere process, but a progress, it is in the life of the folk that we may seek, and perhaps find, the principle of growth. As Goethe said, "Grau, teurer Freund, ist alle Theorie und grün des Lebens goldner Baum."

There is more than that in negro folk-songs, because they voice a something that has been handed down from antiquity, a protest against barbarities committed and endured, and a faith running as a gold thread through the gloomy web of wrongs. All are saturated with African influence and rhythm, yet, at the same time, showing that America enabled the African to say much more than he could say in the country of his origin. The most striking thing about these negro folk-songs is that, though they have come down to us from the days of slavery, and are often the expression of its hard conditions, yet they contain no sense of morbidity, nothing in the way of bitterness, not a word of hatred. The slave did not take any account in his songs of his physical sufferings; sufficient for him was it that God had promised to all who suffered in this world an eternal life in the next. This he believed implicitly; he had been taught that revenge was wrong, and he actually lived the life his missionary teachers taught him, putting his sentiments into his songs. In one of the oldest of negro folk-songs, the melody of which is said to go back to the time of the Pharaohs, there is the following verse:

"Thus saith the Lord,
Bold Moses said:
Let my people go;
If not, I'll strike your first-born dead,
Let my people go."

The African negroes took this song to America, and sang it there in anticipation of their own deliverance. Their masters thought they were singing only words from the Bible, but to every negro it had a personal

significance. At the back of all their folk-songs is the wonderful faith of a people. It is difficult to doubt that it was their faith that at last brought about their deliverance. Certainly, the easel of Time presents many pictures—theory may be grey, but green is the golden tree of Life.

XXV.

Few will be able to gainsay that the act of reading is a necessity in modern society, but the picture of rows of men and women immersed deeply in newspaper or book which presents itself each morning and evening as I make my daily pilgrimage into town suggests often the thought to deplore the invention of writing and printing. Were not men happier and no less wise in the olden days when every male was sowing or ploughing, hunting or fishing, and every woman busy in dairy or kitchen, or with distaff and spindle, from early morn till eventide? There, indeed, after they had eaten and drunk their evening meal, it did them all the good in the world to listen to some itinerant storyteller or minstrel extolling the manly deeds of bygone heroes. There were no intellectuals then to entangle men in the webs of politics or social and other theories, but every one learned by experience both the ways of Nature and of other men. Doubtless that was so for the greater number but, even in the simplest civilizations, there arise a limited few who acquire, and are the repository of a wisdom and knowledge more subtle than can be gathered by any unlettered person and, in the days when the ordinary man did not need to specialize, the holders of wisdom acquired by study were the specialists in knowledge. But now, when everyone must specialize, some wisdom, only to be acquired by letters, is a necessity for everyone. Unfortunately, however, letters have become so easy a medium that unwisdom, as well as wisdom, is daily propagated by their means; nor is that all, because reading, which should be at once a study and a delight, has become for many little better than a drug. A narcotic literature of emotion-stirring facts and ill-digested theories is the product of many of our printing presses, and the evidence of that debasement of literature which is the price we pay for the wide spread of letters.

Notwithstanding, there is no need for pessimism since, with patience, the balance-sheet may be improved by more inspired methods of education, raising the tone of literary digestion. In this matter there is hope for every man and every advance in a literary diet is permanent. As with the body, so with the mind, early training is all-important, and the standard of reading of the next generation of adults will improve in proportion as the feet of the children of this generation are set firmly on the road to humanism, or that quality of the highest part of the human soul to which great literature appeals, as distinguished from the practical or the purely intellectual. But to be successful, this scheme of mental training must not be passive, for the reader, like the listener to music, benefits according

to the intensity of his own co-operation. It is a question of method rather than of technique, and a sound method inspired by a philosophical aim is the ideal. It is not too much to say of any young person who reads at all that what he or she reads in youth moulds the mind for the rest of life, even if they never afterwards return to the particular study. Certain nice points arise, such as: What due should be paid to knowledge? What to curiosity? What to entertainment? and how far historical breadth of view or philosophical depth is the better principle to follow? These questions will be solved by different temperaments in different ways, but, whatever be the road, there can be no doubt that the student should be armed with a strong critical sense. This sense has never been more urgently needed than now when an intolerable clamour echoes still through civilization, and the calm voice of wisdom is not heard. Criticism embodies experience, and experience only comes with years; but that strong critical sense which should grow as the mind matures must necessarily be sown in youth. It is no less valuable than the training in logical scrutiny given by a scientific education, and a mind equipped with either is armed indeed. A danger lurks, however, by the way, and it is over-estimated enthusiasm in each or some new literary pasture attacked, with the result that there is neither a mastery of literature nor a critical sense, but merely an enervated palate to be stimulated only by something new and well flavoured. A mind so unwisely satiated is a damaged intellectual machine, and far removed from the reasonable being with an infinitely varied garden of literary fruits at its disposal, from which it can select according to its mood and constitution. To put the mind in possession of such a desirable garden is, or should be, the true aim of education in literature. The essential tool for its cultivation is criticism, and the young student should be encouraged to use it.

Then there is the case of the old man or woman whose enthusiasms have sobered down, and who has probably formed a nucleus of his literary garden. What is he or she to read? It is, of course, largely a matter of temperament in what direction such a person will turn for literary recreation, but it is also a matter of training and environment in early years. Some will seek relaxation in a return to Virgil, Horace, or Homer; some will prefer to drink from the deep well of English poetry; some will find biographies and books on travel more suitable to their taste; while many others will revel in a highly flavoured detective story. I know many excellent people who are examples of each of these groups, and also I know some who, although they have good libraries full of works by the best authors, constantly complain that they have nothing to read. I often wonder at the cause of this, and have come to the conclusion that in many cases there exists a psychic complex, started far back in youth, when what should have been an enjoyment was turned into an unpleasant task simply because it was presented in a dull way and commented on tediously. To such people the reading of a classic work is frequently a real penance,

because the good books compel weak minds to think and create an unaccustomed exercise. The lesson is, that the mind, like the body, needs to be kept in good training, not always tense but always supple. Most of us have gaps in our literary geography, but we generally keep them to ourselves in shame. I confess that it is only within recent years that I have read Chaucer and Spenser, and I am not a young man. Doubtless, there are many others like myself. The fact is, we should plan and carry out each winter a series of short literary cruises, returning after each one to familiar waters, to old friends, or to the children of to-day whom we cannot afford to neglect. Above all things, we must not put on too much sail in these literary voyages; we may rush through or skim a modern book, but with an older author we must go slower if we wish to penetrate to the levels of his mind and to reconstruct the common intelligence which he addressed in his day. Unless we surrender our emotions, it is useless to read at all; on the other hand, if we let our emotions hurry us on too fast, the memory of our voyage will be blurred or even unrecorded. The camera of the mind may be focused by emotion, but understanding is the lens through which the image is impressed on memory. If we turn over the leaves of our mental records, it is melancholy to note how many of our pictures of past literary excursions are wanting in sharpness of definition. It is by the spark that is within us that we read literature, and not all the fire of God that was in Dante, Goethe, or Shakespeare can control the little spark that is in each one of us who read and respond to the messages which those writers bring home to all who can, or care to, think.

THE LOAD CARRIED BY THE SOLDIER.

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(*Continued from p.* 351.)

XI.—Armies from the Civil War to the Nineteenth Century.

It was left to Cromwell to organize an army on lines comparable to what we regard as modern. His Ironsides were raised to a standard of discipline and training which applied as much to their "Q" as to their "G." He selected the best type of man, preferably freeholders' sons, and clothed and equipped his men—a new departure—on standard lines. In 1631 the first Commission sat on "Uniformity of Arms and Armour," and devised a sealed pattern. The outfit had a good deal more armour in it than had been customary, whence the nickname "Haselrig's Lobsters." The weapons included guns, pikes, etc., and even the bandoliers were standardized (*vide* figs. 10 and 11). In 1640 the pay of 8d. per day was made contingent on marching fifteen miles a day, and one would presume that the iron discipline ensured that each man bore his own equipment. Once again, however, closer scrutiny reveals the truth that *actually* this apparent state of affairs did not represent the case on the march at all: *Archæolog.* XXXVI., 231, reveals that "pack boys" carried the men's packs in the civil war. Presumably pack-boys are beneath the notice of historians other than the uninstructed babblers of reminiscences; but by omitting such details false impressions are inevitable. Sir James Turner in *Pallas Armata*, 1681, throws additional light on the matter, as may be seen from an extract: "Tho' I am fairly of opinion that the soldiers could not carry such burthens as the Romans did of old, yet I should have neither horse nor boy allowed to them; it is too much that the bad custom of later times hath eased most of them of the burthen of defensive armour. . . and therefore both may and should carry his own knapsack and four or five days' provision of meat. . . . Tho' you allow every soldier two pounds of bread and cheese every day—and God knows he gets not so much in four days—suppose he hath a couple of shirts, a pair of stockings and a pair of shoes in his knapsack, and a hatchet, I say all these will not weigh so much as a headpiece and a corselet, and he may well enough be obliged to carry them." We see here the type of criticism suggestive of the amount of clothing to be carried by the soldier, and the commencing realization of the futility of body armour, which before the improving penetrative power of modern missiles gradually disappeared, leaving a soldiery such as we now regard as modern, that is to say, armed with musket (later rifle) and bayonet, clothed in uniform fashion, and equipped with knapsack and

standard accoutrements. As yet, however, "necessaries" were not standardized; and the substitution of clothing only for armour led to the adoption — for near two centuries—of cumbrous dress and equipment, which the critical interest of sane advisers has taken long to eliminate.

FIG. 10.—Pikeman. *Circa* 1650. (After Scott from figure in Tower.)

There is a fairly comprehensible literature on the eighteenth century and Napoleonic wars, from which one may glean something of the conditions of the soldier on the march in those days. He appears to have been awkwardly clad in heavy rigid clothing, and equipped to a considerable weight (*vide* figs. 12 and 13).

The wars in America in the eighteenth century led to considerable alterations in the equipment of the *local* troops, whose commanders found the men unable to compete in mobility with their light-weight opponents,

FIG. 11.—Cromwellian Officer.

FIG. 12.—Infantryman. *Circa* 1700.

and of necessity adjusted the equipment to meet the local situation. This gave rise to many acrimonious disputes between the younger advocates of a really practical kit, and their more hide-bound seniors who insisted on the strict retention of the existing kit. While the former to some extent achieved their aim in America, they were unable to make any real impression on the authorities at home.

Examination of the 18th century Wars in North America furnishes some useful information on our subject. Belcher (*First American Civil War*) estimated that at Bunker's Hill the men carried up to 100 pounds

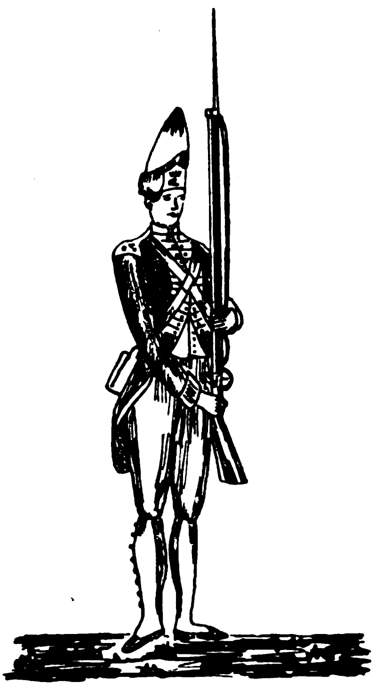


Fig. 13.—Infantryman. ***Circa*** **1770.**

at the parade before action, although this may be an exaggeration. One recognizes here again the presumption that because a man can strut on to a parade ground and bear up his equipment at inspection, he can therefore carry it on the march into action and still usefully employ it. The cumbersome and unpopular equipment of the day was apparently largely modelled on German lines, and called for the following enthusiastic description from Colonel Lloyd : " We borrowed from Germany cross belts which compressed the chest, but had the advantage of throwing the sword to the rear to knock against the calves and the cartridges, and to quarrel with the haversack ; long gaiters which squeezed the legs and stopped the

circulation in that useful member of the foot-soldier; stocks which forced him to keep his head up even with the sun in his eyes; curl papers for the hair, and tight shoes." If to this description we added a true account of the pipe-claying and polishing of the accoutrements and the greasing and powdering of the queue (not abolished till 1808), we should have a fair

FIG. 14.—Grenadier. 18th Century. (After Scott.)

idea of the favour with which the 18th century equipment was regarded by contemporary soldiers; *vide* fig. 14.

In the Indian Wars of North America it proved especially unsuitable, as much so as did the tactics of Europe, which led to many disasters

and to the tragic defeat of Braddock. It was obvious that the natives of the country had to be met and defeated by superior proficiency in their own methods, by specially organized corps, trained, armed and equipped in such fashion as would make them real soldiers of the woods (cf. the Chasseurs and Jägers of Continental armies). This involved setting aside existing regulations, and inevitably led to strong local pressure requiring to be made. As we read in the *Authoritative Account of the Reduction of Louisburg*, 1758, the "British War Office in those days and for many years afterwards ignored the use of Light Infantry . . . but, as a matter of fact, it was always found necessary to improvise light troops for every campaign." Local pressure eventually succeeded; and as a result were formed the first cadres of the Royal Americans (the present King's Royal Rifle Corps or 60th)—regular troops combining the qualities of scouts and expert woodsmen with the discipline of trained soldiers. Forest war required the existing full dress uniform to be discarded for something less conspicuous and more suitable. Bouquet, on whom fell much of the organization of the new units (*Butler's Annals of the King's Royal Rifle Corps*), desired his men to dress like the Indians, and advocated "that the troops destined to engage Indians must be lightly armed, clothed and accoutred. The clothing should consist of a short brown cloth jacket, tanned shirt, short trousers, leggings (puttees), and socks and mocassins, with a knapsack for provisions and an oiled surtout." He also advised short fusils (rifled), at a time when the troops still carried the cumbersome smooth bore, with bayonet fashioned like a dirk to act also as knife, a small hatchet, and a leather water-bottle. He assumed a day's ration of one and a half pounds of meat and one pound of flour, and insisted that men "must be trained to carry burdens proportionate to their strength." Not all of Bouquet's recommendations were accepted, but in a description of Lawrence's Light Brigade under Amherst we find the troops were dressed ". . . . some in green jackets and drawers for the easier brushing thro' the woods," and were armed with fusil, cartouche box and powder horn. The Royal Americans, later the King's Royal Rifle Corps, have never lost the traditions of these early days, although the standardizing of equipment of later ages has once more removed them from the category of true light troops.

The effect of the cumbrous late 18th century equipment may be seen in contemporary records and in the tremendous amount of straggling during the Napoleonic Wars (*vide* the descriptions of Moore's Corunna Retreat), whenever any real test of marching powers was involved. Riecke gives a striking picture of a march of the Prussians in 1778, the men carrying a load of eighty pounds. "In one night the soldiers looked as if they had aged ten years, almost at every step lay a fainting man, and entire troops lay on the road-side. In such fashion the entire army marched in four days to Dresden, and getting there exhausted even to death found the Saxon Army fresh and lively." He describes further how

"they only recovered after a long time . . . when the pack which confined the chest so heavily was removed." And yet this occurred among an army selected chiefly for its massive physique: Frederick II particularly profited by the knowledge of the effect of intimidation and selected "a line of soldiers of huge size and imposing aspect." In the tumultuous Napoleonic era, when most of the countries of Europe were turned into armed camps, we find the greatest amount of critical observation on our own and foreign armies, and a wealth of memoirs from which to reconstruct the scene.

FIG. 15.—Wellington's Army. Infantryman on the march. (After Oman.)

Oman (*Wellington's Army*) gives a complete description of the early nineteenth century soldier which may be summarized as follows (and also excellent plates of infantry of the period). Blue gray trousers had been issued in 1808 as service substitutes for the older buttoned gaiters. This was a great improvement, as the gaiters, taking twelve minutes to button, were not infrequently worn for days on end, with considerable resulting discomfort and uncleanliness. The short coat still had many eighteenth century features, but was not improved by the tight collar and leather stock, "an evil device which constricted the neck and tended to apoplexy," and which was, indeed, usually thrown aside in action. Bayonet and cartridge box were supported by a broad leather belt with heavy brass plate. The very heavy oilskin knapsack was supported by straps under the arms. Including the canteen and haversack, Oman estimates the

whole as weighing some sixty pounds. But it should be recalled that Wellington was no "stickler" in regard to uniform, and frequently employed even French clothing for his men. He looked to results rather than appearance.

Fig. 15 suggests the appearance of one of Wellington's infantry on the march; and fig. 16 a rifleman of the same period, while various statues such as those at Hyde Park Corner, London, commemorate the equipments worn by the soldiery of the day.

Fig. 16.—Rifleman of Peninsular period.

Jackson (*loc. cit.*) had some sharp commentaries on the clothing of our own troops. Speaking of 1804 he writes, "The dazzling colour of the uniform, the variety of facings and profusion of ornaments and polished accoutrements make a gorgeous display of millinery taste and military error—*a coup d'œil* at variance with utility." He contrasts favourably the Russian clothing (of which we have favourable mention also by Marshal Keith) fashioned for convenience and utility so that the soldier was at ease, and not fettered even when he slept accoutred. (There is much to admire in the Russian uniform even to-day.) His critical eye—suggestive of the constructive ideas of half a century later—also noted favourably the Spanish uniform, "the men having the appearance of being well selected, equipped and accoutred; not so gilded as the high dressed battalions of the professed military sovereigns, but perfectly well adapted for service in the field." As regards the equipment and accoutrements of these days,

it appears that most of the European armies were fitted out on somewhat similar lines, based perhaps on the well-known postulate of Napoleon that the soldier's essential requirements comprised his firearm and ammunition, his trenching tool and knapsack, and four days' rations. But we find Jackson (*loc. cit.*) complaining that "the equipments are often multiplied capriciously"; "the knapsacks are crammed with necessaries so as to load the foot soldier like a packhorse, oppressing him by its weight and consuming some of that power which ought to be reserved for military exertion"; and, in general, referring to these so-called necessaries, "There is not more personal comfort, there is inconvenience from the possession of quantity: superfluity of baggage is a common error in the British Service." The total weight of the British infantryman's load in the Peninsular War is often quoted as sixty to seventy pounds, and Wellington himself later estimated it at Waterloo as fifty-six pounds; while the French averaged fifty-eight pounds. Kirchner (*Military Hyg.*) gives the French load at that time as fifty-three pounds, of which the weapons comprised nine pounds. Other figures are available from various sources, and the net weight appears to have varied somewhat. From the data available I take sixty pounds to have been the average British load, and fifty pounds the average French. About this time the whole possessions of the soldier, other than personalia, were standardized, so that total weights can, henceforth, be accurately estimated.

Let us see now how these loads were carried. In the first place they appear to have been sufficiently heavy to render the troops incapable of any unusual marching power or special mobility. The evidence is all the reverse. An occasional outstanding march therefore commands the more conspicuous notice, and perhaps the most noted was that of the Light Brigade (43rd, 52nd and 95th Foot), who have long been credited with covering sixty-two miles (to Talavera) in twenty-six hours under a sixty pound load. (Napier.) This march, admirable in its execution (only seventeen men fell out), and in its results, requires however some analysis. We find that the brigade had been specially trained in marching for the previous month, and that fifty of the more weakly men were left behind; while as regards their load, the troops carried two-pound bags and not framed packs, and there must be very great doubt as to the weight of the scanty contents. Parkes (*Mil. Hyg.*) quotes a conversation of Lord Clyde with his colleague Longmore in which the former described seeing the men on the march and stating that *actually they only carried a shirt and a spare pair of boots or of soles* in their bags; while it is probable that only a part of the ammunition was carried. It is evident, therefore, that this undoubtedly fine march cannot be used as evidence of the power of men to fight after a long march under a *heavy* load. Even under ordinary conditions of march however the load would appear to have been cumbersome, and in these wars, for perhaps the last time, one must allow not only for the official load, but for personalia. (See earlier section.) Under

such circumstances it is not surprising to find a tendency for individual men to straggle (and on more than one occasion such stragglers gave away Wellington's plans) either in search of plunder, or from exhaustion; while auxiliary transport was employed wherever possible (donkeys, country carts, &c.). Again one must reckon with the womenfolk (so troublesome and obdurate to Wellington) and other camp followers, referred to in the various memoirs of the period.

Lejeune, for instance, in his *Memoirs*, illustrates the conditions of the march by describing a detachment as follows: "The Captain rode first on a very fine horse, then came his wife riding on a mule. Beside Madame walked her Irish nurse. A Grenadier, the Captain's servant, came behind, and last in the procession came a donkey loaded with much miscellaneous baggage; it was guarded by an English servant in livery mounted on a sturdy cob." While, as regards the womenfolk in general, we may recall the extract from the *Rough Notes of an old Soldier:* "The multitude of soldiers' wives stuck to the army like bricks; averse to all military discipline, they impeded our progress at times very much, particularly in retreats. They became the subject of a General Order for their own special guidance. They were under no control, and were always first *mounted up* and away, blocking up narrow paths and checking the advance of the army *with their donkeys*, after repeated orders to follow in rear of their respective Corps, or their donkeys would be shot." General Foy, in his *Memoirs*," remarked: "To look at the mass of impedimenta and camp followers trailing behind the British, you would think you were beholding the army of Darius. Only when you have met them in the field do you realize that you have to do with the soldiers of Alexander."

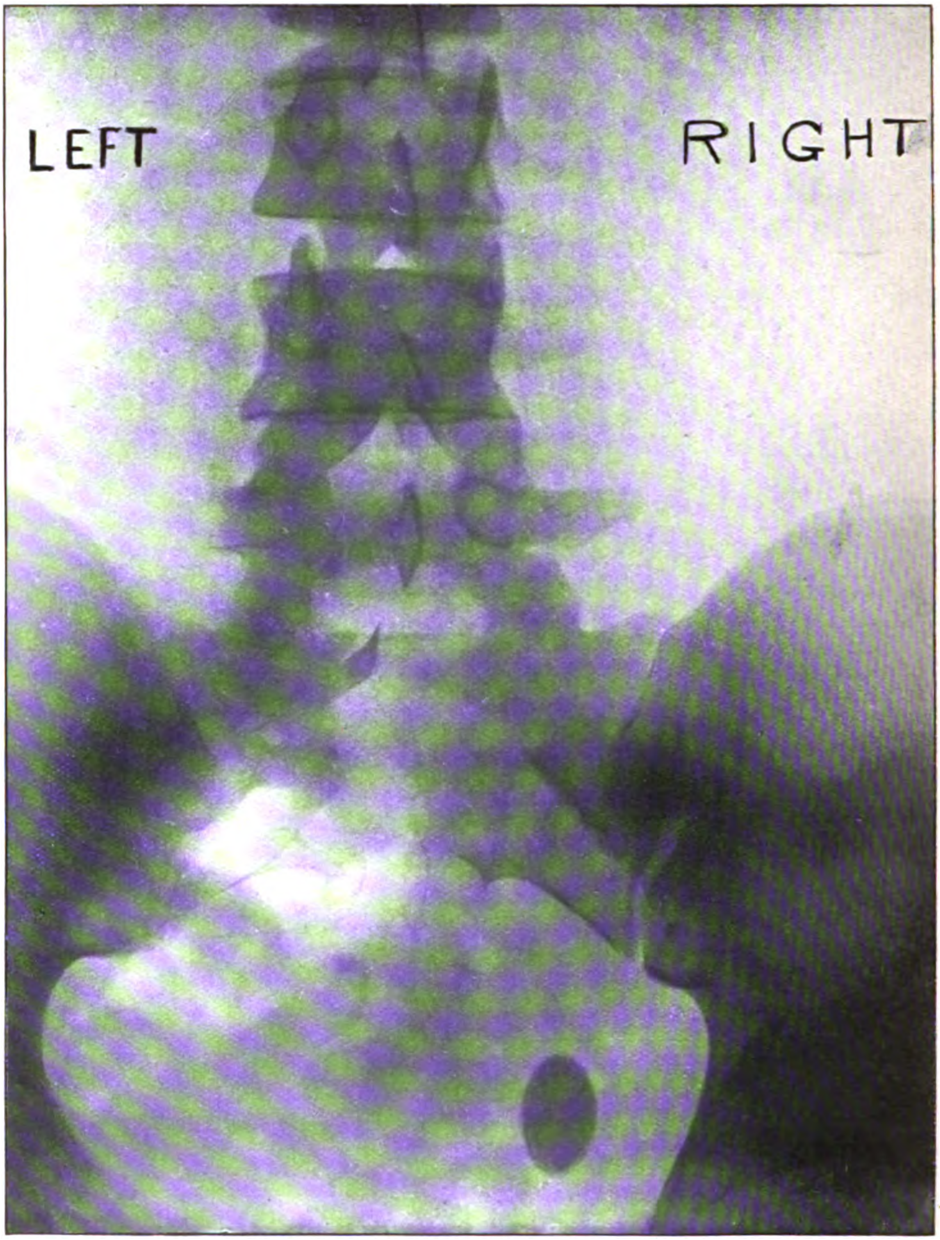
That this auxiliary transport and personnel relieved the foot soldier of some of his load is indisputable. The size of the British train was always a subject of comment to foreign observers, and while the records of the Napoleonic wars show the British infantryman of the period to have been in no way open to criticism of his fighting abilities, it seems fair to remark that he showed the usual British disinclination to march loaded without assistance; and where compulsion was offered tended either to straggle or to discard his excess load, facts which appear clearly from an examination of Wellington's dispatches and letters and other contemporary literature (*vide* also Wellington's letter to Bathurst, August, 1813, after the campaign against King Joseph, when he refers to the deterioration and need for renewal of equipment after a period of marching). Indeed Wellington found it constantly necessary to issue instructions in training the troops to march with a load, on account of the poor marching which was his constant difficulty. Napier (*Pen. War, IV. Ch. VI.*) describes the severe losses from straggling on the march of Sir J. Moore's Corunna force, not only among the main force, but also among the Light Brigades (sic!) who travelled by the Viga Road. The latter were not pursued, made no *forced* marches, slept under cover and were well supplied,

and yet left in proportion to their strength as many men behind as any other part of the army! So much for the effects of the equipment of the day!

I have already indicated how Napoleon relieved his men, wherever possible, of their marching load. Thus on his march to the Rhine (to Austerlitz) in 1805 he employed relays of carts and we find him for instance (according to the records of the Comte de Ségur) directing the Mayor of Lille to "feast my divisions on the march, and organize chariots to double their marches." Not only so, but his magnificent march over the Syrian desert was (as I am informed, though unable to quote reference in the official French volumes) made possible by the supply of one donkey to every three men, for carriage of their belongings; and Killick (*French Army in Egypt*) refers to the assistance given by camels, on each of which 10 men (sic!) were loaded. On that occasion also we must remember that Napoleon depended largely on securing supplies locally.

It is evident, then, that not yet does history tell us much as to what the ordinary soldier actually did show himself capable of carrying without detriment to his consequent fighting power; the campaigns of that day usually included considerable halts, and battles did not involve so much tactical mobility as later. Many figures and illustrations of the soldiers of these days are extant.

(*To be continued.*)



To illustrate "Case of Stone in the Ureter," by Captain J. H. M. Frobisher, R.A.M.C.

Clinical and other Notes.

CASE OF STONE IN THE URETER.

By Captain J. H. M. FROBISHER.
Royal Army Medical Corps.

The patient was admitted to hospital for an attack of acute abdominal pain. He stated that for the last five years he had had similar attacks at intervals of a few months. The pain was situated in the right loin, came on gradually and lasted about a week when it gradually subsided. Each attack was similar. The pain was not very severe in character, but great tenderness in the loin existed. The temparature was normal on admission. Urine normal. No blood, pus, or albumin. X-ray examination revealed a stone in the lower end of the right ureter.

Operation: Open ether. Trendelenburg position.

Abdomen opened in middle line. General abdominal cavity packed off. Stone found in position shown by X-ray. It was easily movable in the ureter, which was very dilated for about three or four inches on either side of the stone. The peritoneum over the ureter was incised, the ureter raised, and held in two silk slings one on either side of the stone. The stone was then removed through an incision in the ureter, the incision next being closed by Lembert sutures. The ureter was replaced and the peritoneum sutured over it. The kidneys on palpation appeared to be normal. There was no apparent enlargement, and the pelvis was not dilated. The appendix was found to be normal, but was removed. Abdomen closed in usual manner.

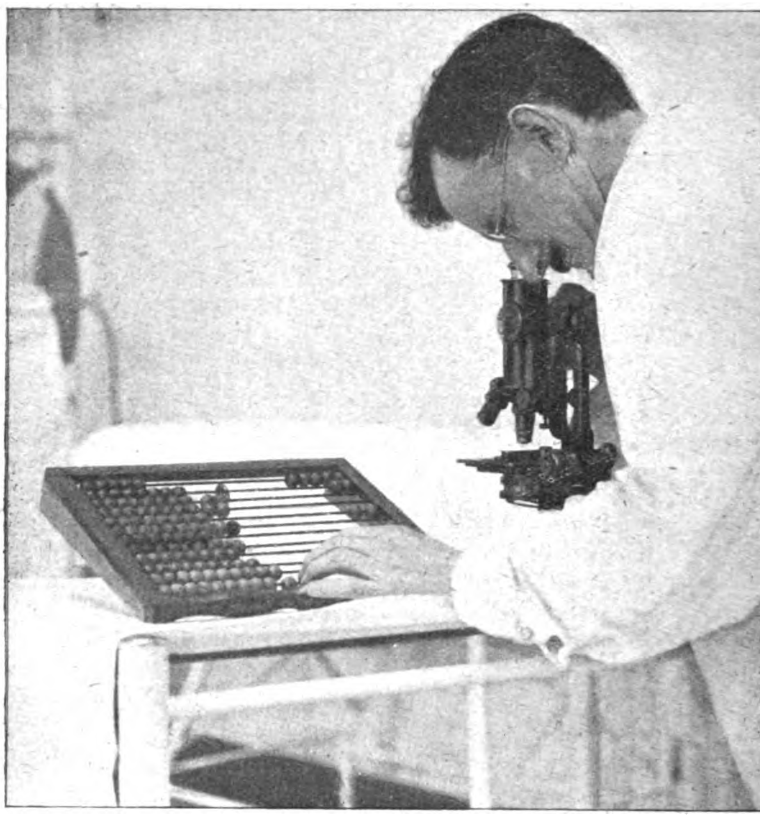
After History.—Considerable hæmaturia after operation. Examination of urine now showed presence of blood, and a few pus cells. Urine became normal after eight days, and convalescence was uninterrupted.

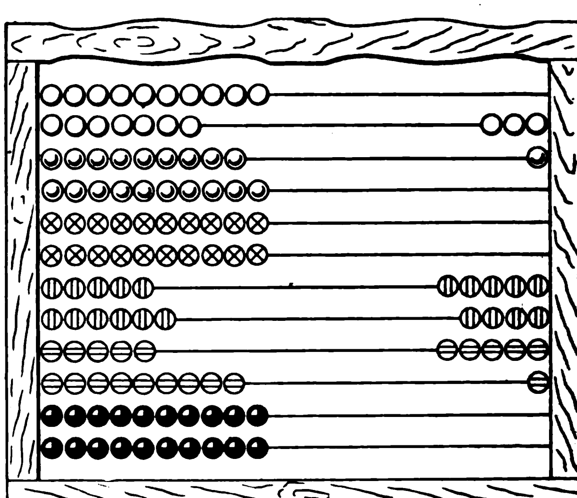
THE ABACUS IN THE LABORATORY.

By Major F. W. W. DAWSON.
Royal Army Medical Corps. D.A.D.P., S.W. Area.

Nearly ten years ago, a patient in the officers' ward at Kasauli, seeing me toiling through a blood count with paper and a pencil, suggested the use of an abacus. I have forgotten his name, but I have remembered his suggestion with gratitude.

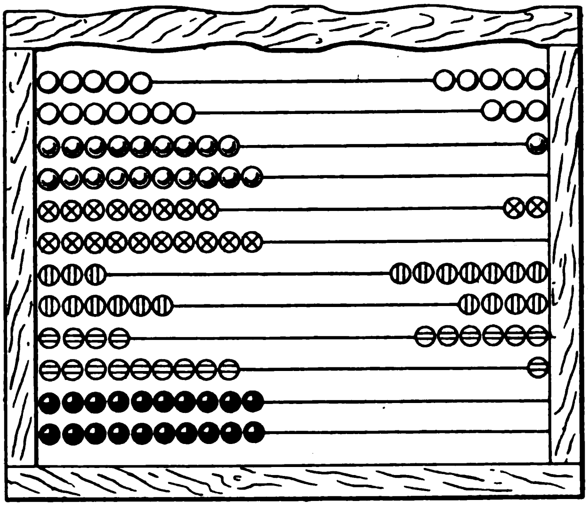
I have tried several patterns. My trail through the various laboratories in India can be tracked by the abacus. The one I am now using is an ordinary school abacus, which was obtained from the Kindergarten Department of the Baptist Mission Store in Rangoon. It has twelve rows, each holding ten counters. It is mounted on chocks, so that it lies at an angle of about ten degrees to the surface of the table. It is placed to the left of the table and is worked by the left hand. This leaves the right hand free to work the microscope. With a little practice it is not necessary to take the eye off the microscope. This presents a great

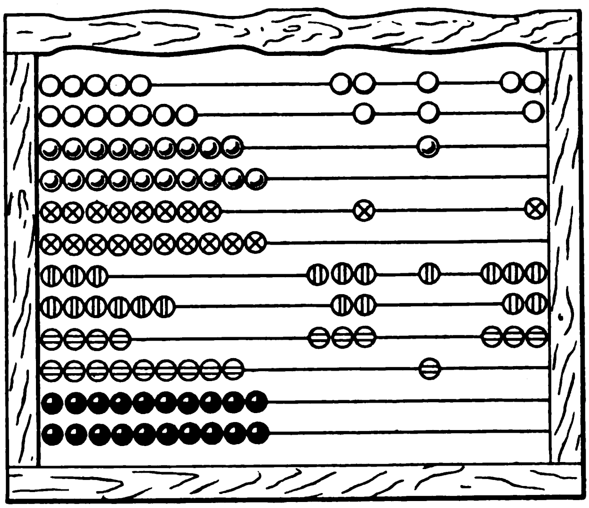




contrast to the usual process of picking up a pencil and making a mark on the paper.

A differential count is done in the following manner: The first three rows record polymorphs. The first records units, the second tens, and the third hundreds. The fifth and sixth record eosinophils, the seventh and eighth





lymphocytes, the ninth and tenth large mononuclears. This leaves the fourth, eleventh and twelfth for abnormal cells.

Where one is dealing with leukæmias, an adjustment of rows can easily be made.

With this apparatus it is easy to count the exact number required. I usually count 200 leucocytes. If there are three varieties recorded, I watch the rows

recording tens until their total reaches 180. I then count all the counters in order to ascertain how many more I require to complete 200. In the illustration I require ten. These are counted one by one till the number is complete. If four varieties are recorded it is then necessary to make the complete count as soon as one has reached 170, and in the case of five varieties 160.

In the completed count shown, the result is as follows:—

Polymorphs	135
Eosinophils	2
Lymphocytes	47
Large mononuclears	16

The counters are then divided into two halves by a touch of the fingers, giving the percentage count as follows:—

Polymorphs	67·5
Eosinophils	1
Lymphocytes	23·5
Large mononuclears	8

In a vaccine count, the first four rows are used for the red cells, and the organisms are counted on the rows beginning from the seventh and used for units, tens, hundreds and thousands, respectively. A quantitative count is done in a similar manner.

It has been of considerable assistance in the compilation of returns, particularly where one has to pick out data from a register.

By adding two counters to the eleventh row it may be used to assist in the keeping of the laboratory accounts.

SUGGESTED IMPROVEMENT ON THE PRESENT SHELL DRESSING.

By Captain R. MACKINNON.

Royal Army Medical Corps (Special Reserve).

To apply the present shell dressing the clothing must first be cut away or removed, thereby exposing a large area of the patient's body, and a good deal of movement and handling of the patient is required. For a patient already suffering from shock due to a wound, to diminish unnecessary exposure and handling is an important matter.

A shell dressing that could be applied to a wound and bandaged over the clothes would avoid this delay and exposure—in winter time and cold nights of importance.

Such a shell dressing may easily be made by attaching the bandage to the centre of the pad only (as in diagram) instead of the whole of the surface of the pad.

To apply this dressing the method is as follows:—

(1) Enlarge the hole in the clothes made by the bullet or piece of shell by slitting the clothes over the wounded area with scissors.

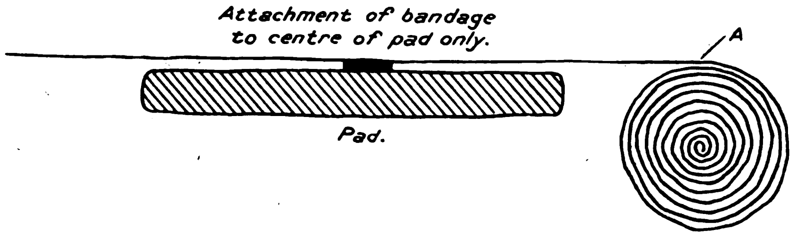
(2) Pull apart the two layers of clothing and expose the wound.

(3) Tuck the pad in under the clothes and spread it over the wound.

(4) Fold the two layers of clothing over the pad and bandage outside the clothing.

If applied carefully there is no reason why the under surface of the pad should be allowed to touch the clothes or be contaminated in any way.

There is very little exposure of the skin, and bandaging this way over the clothes is infinitely better and requires less handling of the patient.



While working in the front line where means of warmth are lacking and where the dangers of shock are paramount, I have been greatly impressed by the advantages of this method in wounds of the thigh and trunk.

Note.—In the present shell dressing the bandage is too securely stitched to itself to allow of rapid adjustment. I would recommend one loose stitch or none at all.

MALARIA AND UROBILINURIA.

By Major W E. HOME.
Royal Army Medical Corps.

A case of masked malaria in England which was diagnosed in consequence of urobilinuria may be worth recording.

A trumpeter, aged 17, fell ill on July 27, 1921, and on July 29, 1921, complained of headache and faintness and was sent into hospital.

He ascribed his condition to frequent sea bathing (four times a day) in hot sunshine. He had been seven years in India, and returned home a year ago, but had no history of malaria.

On admission temperature was 100° F., at night 102° F.

On July 30 his urine was brownish, and showed an unusual amount of urobilin; indican was also present, no bile pigment. The afternoon temperature was 104° F.; he had no rigor, but sweated subsequently.

On July 31 urobilin was still further increased, temperature normal. In a blood film, one suspicious *Plasmodium vivax* was seen (four were found later in the other film by the D.A.D.P.). All this time he was getting a diaphoretic mixture only.

On August 8 temperature was 103·4° F., at 10·00 hours, no rigor or other discriminating symptoms, and no parasites in films then taken, hæmoglobin 100, red blood corpuscles 6,500,000, white blood corpuscles 8,000, slight cough, skin flushed, headache. At 14·00 hours temperature 105·6° F.; quinine ten grains, t.i.d. was then ordered, and aspirin for headache. Temperature fell and was subnormal all next day.

Had it not been for the urobilinuria, in view of the absence of characteristic symptoms, one would have hesitated to give quinine since that would preclude any subsequent attempt at laboratory diagnosis, and the patient should take

quinine for ninety days. A single *P. vivax*, not too well stained, is little to go on, and laboratory report was not available till later.

Urobilin is excreted in quantity after any large destruction of red corpuscles, and they are most rapidly destroyed in malarial attacks. Urobilin is most conveniently recognized by the direct vision spectroscope and the amount roughly estimated by the depth from the top of the conical glass that shows the absorption spectrum. I prefer to add five cubic centimetres of amyl alcohol to ten cubic centimetres of urine acidified with HCl, shake well and allow to settle, when the resulting colour of the alcohol suggests the amount of urobilin present, as does the colour of the chloroform in testing for indican.

I would like to mention here a test for bile pigment in urine which I find very delicate and convenient. It is that of M. Albert Rolland (*Arch. de Méd. Militaire*, April, 1921, p. 415), iodine 1, potassium iodide 1, water 50, spiritus rect. 150.

It floats above the urine, gives a sharp interface, and a green line between if bile pigment is present.

Echoes from the Past.

REPORT ON THE LITTLE COCOS.

By Colonel A. LANG BROWNE.
Army Medical Service (R.P.).

On February 19, 1882, the party organized and headed by Major (afterwards General) Protheroe, C.S.I., Officiating Chief Commissioner Andaman and Nicobars, after their inspection of Saddle Peak, North Andaman, proceeded to and landed on the island of the Little Cocos. The object of the visit to the island being to ascertain if it would be a suitable place to send a party of convicts to gather the nuts during the dry season. The matter contained in this report was gathered during a short stay of a few hours only at the island and is therefore of necessity more or less limited in regard to minute details.

General Description.—The island of the Little Cocos lies twenty-eight miles to the north-east of the Andamans group in longitude 93° 13′ east and latitude 14′ north and at a distance of ten miles in a south-westerly direction from the island of Great Cocos.

The island is about 3½ miles long and from ½ to ¾ of a mile broad and 7¾ miles in circumference. It is rather flat, but running through its centre from north to south there are two ridges of low hills from eighty to 160 feet in height, and along the east coast small hills also crop up here and there close to the sea beach, particularly towards the north-east where they run together and form a low rocky cliff overlooking the sea for some 300 or more yards. The sea beach with the exception of 800 yards on the east coast (where heavy stone boulders and gravel

exist) is composed of beautifully white sand mixed with brilliantly coloured shells and coral debris, which has by the constant action of the flow and ebb of the tide been washed or silted up on to the edge of the original coral reef until it now forms a sandy bund of from 4 to 6 feet high and 50 to 100 feet broad all round the entire island. At both the north and south extremities of the island, rocks and extensive coral reefs run out into the sea for some 400 or more yards; the south reef is the larger, and extends out for a very much greater distance. Immediately beyond its most southern edge the water is very deep and so clear that the white sand at the bottom can be easily seen and also the fishes, crabs, etc., moving about. At this point at a great depth the wreck of a three-masted ship with sails set, lies on its side at the bottom. It must have lain where it is for many years, as no account of a ship having been wrecked on this reef is obtainable. Being at so great a depth down it appears to have escaped damage by storm, or by the ebb and flow of the sea, but possibly it is covered with barnacles.

Anchorage.—According to the season of the year good and sheltered anchorage for even large ships can be obtained to the lee of the island either on the east or west coast.

Between the sandy bund which forms the beach, and the high land towards the centre of the island the ground for a varying breadth of from 100 to 300 yards is flat and only slightly above sea level. This flat land no doubt has for its foundation the original coral reef, which in ages past first grew up and surrounded the high land which now forms the centre of the island. The soil is composed of sand and coral debris mixed with earth and decayed vegetable matter washed down from the hills. During the dry season of the year this flat land is hard and firm, except at the south end of the island where it is more or less swampy owing to the rain water from the hills collecting at this point and forming a shallow lake, but in the rainy season of the year it must be all mostly under water, as the sandy bund forming the beach prevents to a great extent the free escape into the sea of the rain water from the hills, which therefore collects, and forms a temporary muddy swamp during this period.

The high land which rises from the inner edge of the flat ground above described consists for the most part of two ranges of low hills running north and south and varying in height from eighty to 160 feet. The soil covering these hills appears to be composed of decomposed lava, yellowish clay and black vegetable mould. Many small dry watercourses run down from and intersect the hill sides, and empty their contents during the rains on to the flat ground above alluded to.

Vegetation.—Coco-nut trees grow all along the sea beach and over the flat land and form a thick belt which surrounds the island to the depth of 100 to 250 yards. The remainder of the island is covered by light scrub jungle with coco-nut, sago palms and other large trees of many varieties interspersed through it. On the eastern coast there are some magnificent

trees, and the vegetation on this side is most luxuriant. A few mangrove trees grow in the swampy ground at the southern extremity of the island.

Natural History.—The wild pig and iguana are plentiful. Pigeons of many varieties, teal and small birds abound.

Population.—There are no people living constantly in the Little Cocos, but Burmese visit the island regularly every year to gather coco-nuts.

Water.—With the exception of the swampy lake at the south end of the island no water was found, but on the north-west coast there are the remains of an old well supposed to have been sunk by some nut gatherer. It is however at present almost choked up and nearly dry. Were proper wells sunk, no doubt, an abundance of good water would be found. A large and good supply of water might also be collected by leading into a tank a few of the many streams running down from the hills during the rainy season, and so store a sufficient supply to last throughout the dry season.

Climate.—Owing to its position the Little Cocos is exposed to the full force of both the south-west and north-east monsoon, and therefore during one part of the year (i.e., when the south-west monsoon is blowing) the climate must be very damp, muggy, and more or less unhealthy. While on the other hand the climate should be cool, bracing and healthy throughout the north-east monsoon, more so towards the middle and end of the monsoon, when the swampy and damp places will have become dried up and so to a great extent non-malarious. The healthiest season of the year might therefore be said to be the period comprised between December 15 and March 31, when the island is comparatively dry and free of stagnant water.

Temperature.—The maximum and minimum temperatures taken on February 15 and 16 respectively were : maximum, 85° and 82°; minimum, 81° and 79°.

Conclusion.—As regards the Little Cocos being used as a convict station during the nutting season, that is from December 1 to March 31, there is no apparent reason why its climate should prove unhealthy were the following recommendations carried out, viz., (1) a good wooden barracks raised on piles and built on one of the hills situated towards the north-east end of the island ; (2) wells sunk so as to give a sufficient supply of good water ; (3) a good ration containing a large amount of vegetable matter with salt, etc. ; (4) clothing made suitable to the climate which is much cooler than that of the Andamans generally.

About the time the above report was written in February, 1882, a German naturalist visited Port Blair with the object, he said, of searching for a particular orchid which he considered might be found growing in the jungles of the Andaman or Cocos Islands. He spent several days exploring these jungles, and, no doubt, in his supposed searchings for this unknown specimen of the orchid family came across the two fine land-locked harbours of Port Cornwallis and Stewart's Sound at the base

of Saddle Peak in the North Andamans, and, of course, must have noticed how the entrance to each of these fine harbours is so screened by the formation of the land and the high tree jungle that, looking from the sea it is, until getting quite close in, impossible to see any opening, and how even a very large vessel once inside the harbour would be so hidden that a ship passing close along the coast would never detect her. No better base could there be for a pirate vessel to lie low in and steal out of, to attack merchant and trading vessels on their way through the Indian Ocean than either of these harbours. Now, may not the German mystery ship, the "Little Emden," have used these harbours when making her attacks on the Madras, Burmah, and Penang coasts during the late war, and was she not chased by H.M.S. "Sydney," and wrecked on North Keeling or Cocos Island? And may not the very simple-minded naturalist have found in these harbours what he was really in search of? Possibly he may have found the wonderful and unknown orchid, too, but I never heard that he did.

Reviews.

HYGIENE OF COMMUNICABLE DISEASES. By Francis M. Munson, M.D. Published in New York by Paul B. Hoeber, 1920. Pp. xiv + 793 with 36 illustrations. Price $5.50.

The aim of this book is "to present in a concise and readily accessible form the information now available concerning epidemiology and the management of the communicable diseases, ashore and afloat."

The first chapters are devoted to a discussion of the causes, methods of spread, and prophylaxis of communicable diseases in general. Then follow chapters dealing with the application of these general principles to special conditions, military, naval, municipal, rural, school, prison, industrial, arctic and tropical communities, being separately considered. A novel chapter in this part of the volume is one dealing with "Sanitary Measures following Great Disasters."

In Part II each disease is considered separately, the diseases being grouped according to their method of spread.

The general arrangement is logical and well thought out, paper and printing are good, and effective paragraphing with bold type headings facilitates reference. It is therefore the more disappointing to find important matters dismissed with inadequate discussion. For example, bacterial food poisoning is dealt with in three-quarters of a page, although two pages are devoted to the less important subject of botulism, while important communicable diseases (e.g. encephalitis lethargica and schistosomiasis) are omitted altogether.

Several of the statements in the text are open to obvious criticism. Thus the author states (p. 753): "Cases of chicken-pox should be reported, if for no other reason than that it is frequently mistaken for small-pox." Surely the converse possibility, mistaking a sporadic case of smallpox for one of chicken-pox, constitutes the real danger. And in these days when "vitamine" is a household word, it is surprising to read (p. 13) that a diet deficient in potassium salts causes scurvy.

Upon the whole, the volume falls short of success as a satisfactory exposition of its subject.

J. A. A.

THE CLINICAL EXAMINATION OF DISEASES OF THE LUNGS. Brockbank and Ramsbottom. London: H. K. Lewis and Co., 1921. Pp. viii + 88. Price 4s. 6d.

This little book, primarily intended for the use of students at the commencement of their clinical studies, gives a clear and concise account of the essential points in the examination of the lungs.

To more advanced students and practitioners, however, its persual would serve as a handy reminder on many points become unfamiliar in the course of time or through lack of practice.

AN ACCOUNT OF THE CAVALRY OPERATIONS IN PALESTINE AND SYRIA, 1917-1918. By Lieutenant-Colonel the Hon. R. M. Preston, D.S.O. London: Constable and Co., Ltd., 1921. Pp. xxiv + 356. Price 21s. net.

General Sir H. Chauvel in his introductory note to the above work points out its great value to students of military history.

There can be little doubt that this book which has been so carefully and accurately written will be of the greatest interest to soldiers in general, and cavalry soldiers in particular.

The author has a very clear insight into military matters and deals with the doings of the various branches of the Service in a straightforward and readable manner.

Readers will be struck by the magnitude of the campaign and the many difficulties successfully overcome. It is interesting to note that the whole of the cavalry work was carried out almost entirely by irregular troops, Yeomanry and Australian Light Horse, the only regular cavalry being the Indian Regiments which, however, did not arrive on the scene until May, 1918. A fitting tribute is paid to the work performed by the Royal Army Medical Corps. The maps and photographs are very descriptive.

ON BONE-FORMATION: ITS RELATION TO TENSION AND PRESSURE. By Dr. Murk Jansen, O.B.E. London: Messrs. Longmans, Green and Co., 1920. Pp. 114. Price 20s. net.

The essential purpose of this book is a discussion of the time-established doctrine which attributes to both tension and pressure the power of causing and promoting bone formation.

The author argues his case with considerable authority, and illustrates his points with frequent and excellent plates.

Whilst to most the work is chiefly of academic interest, to the bone surgeon it might well serve to throw light on some of his special problems.

ESSENTIALS OF MEDICAL ELECTRICITY. By Elkin P. Cumberbatch, M.A., B.M., B.Ch.Oxon., M.R.C.P. London: Henry Kimpton, 1921. Fifth Edition. Pp. xv + 388. Price 17s. 6d. net.

This fifth edition enhances the high reputation which had been gained by previous editions.

Although the title only claims to deal with essentials, the student or practitioner who reads this book carefully will have gone far in mastering the complex subject of medical electricity.

The subject is dealt with in fifteen chapters, and a good index is appended.

Chapter V, which deals with ionic medication, has been modified in this edition, and the value of the method is fairly stated, while no extravagant claims are made as to its therapeutic effects.

Chapter X, on the testing of the reactions of muscle and nerve, is clearly written, and the value of condenser discharges is emphasized.

Chapter XIV is an index of electrical treatment, and a long list of diseases

is included. We are rather sceptical as to the value of electrical treatment in some of these conditions, yet, in the great majority of the diseases which are included in the list, the claims for benefit from electrical methods are clearly substantiated.

It would be an improvement if the order of the chapters was altered so as to bring Chapter XV, which deals with physical principles, to the beginning of the book. This alteration would be a great help to the novice on the subject, and to practitioners whose knowledge of elementary electricity has grown rusty.

The book is excellently produced, and contains eleven plates and seventy-six illustrations.

The author is to be congratulated on his work, which has given to the profession the best short manual on the subject which has been written.

Materia Medica and Pharmacy. By Reginald R. Bennett, B.Sc.Lond., F.I.C. London: H. K. Lewis and Co., Ltd., 1921. Fourth Edition. Pp. xxiii + 263. Price 7s. 6d.

As stated in the preface, this book is intended to present a concise account of the drugs, chemicals and compound preparations of the British Pharmacopœia, a purpose which is admirably fulfilled. The arrangement is excellent, and there is no overloading of the text. The appendix on incompatability is particularly useful.

One criticism, and that in view of the expressed intention of the work perhaps superfluous, is the absence of any brief summary of the chief extra-pharmacopœial preparations. Their employment now enters so largely into medicine that a working knowledge of their uses is indispensable to the practitioner.

Correspondence.

RELICS OF HISTORIC INTEREST TO THE ROYAL ARMY MEDICAL CORPS.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

Sir,—Two relics of historic interest to the Royal Army Medical Corps in the late war have recently been sent to the Museum of the Royal Army Medical College, viz., a wooden board labelled "No. 1, Harley Street," and a "Walking-Wounded" directing flag with corps or divisional sign (a blue triangular flag with small white circle in the centre), made of tin; the latter riddled with bullets.

Both of these were collected from behind the British line on the Bethune-La Bassé road shortly after the Armistice.

It is known that even as early as December, 1914, the trench line containing the R.A.P.'s in this area was called "Harley Street," and Harley Street it remained until the end of the war. On the whole of the British Front probably no position changed less than this area, and there was probably not a division in France that did not, more than once, assist in holding this part of the line. For these reasons, if for no other, "Harley Street" is probably more widely known to the Royal Army Medical Corps in France than other similar positions, and its name must awaken many and varied memories of those days.

Officers who are able to afford any information as to the history of origin of

these signs, or can give any interesting notes in connexion with them, are requested to forward such information to the Commandant, Royal Army Medical College, so that a short account can be placed on record with these war souvenirs.

I am, etc.,

Royal Army Medical College. H. A. HINGE, *Colonel,*
October 13, 1921. *Commandant.*

ATROPINE IN INFLUENZA-PNEUMONIA.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

DEAR SIR,—I have recently had under my charge a patient suffering from the above, and whom I treated with liq. atropinæ in the acute stage with the following results:—

The patient on the first day had high fever, rigors, sharp pain in the right side, suggesting acute pleurisy, and cough, at first without expectoration. He also complained of vague pains in all the more important joints.

Next day he began to expectorate. The sputum at first was streaked with blood, but it soon became more abundant, and more fluid consisting of thin mucus with a large quantity of dull red blood. The sputum, examined at this stage, consisted almost exclusively of pavement epithelial cells, polymorphs and red cells. There were also large numbers of very small Gram-negative bacilli, suggesting Pfeiffer's bacillus, and a few pneumococci.

On examination of the chest, definite signs of dry pleurisy were present, the first evening, on one side.

Within twenty-four hours this spread to the opposite side, together with definite signs of extensive catarrhal pneumonia of both bases. The patient was in real distress, and almost suffocating at times. Apart from extensive and repeated poultices, he was prescribed the following: liq. atropinæ one minim, tr. benz. co. half drachm in half an ounce of suitable vehicle, t.d.s. He also received five minims of liq. adrenalin hypodermically.

Within twenty-four hours the improvement was remarkable, especially in the sputum, which was considerably reduced in amount, and completely changed in character, to thick muco-purulent, almost nummular. The general condition of the patient was proportionately also improved, also the pulse, respiration, etc.

Atropine treatment was stopped immediately the more familiar physiological effects of the drug began to appear, viz., mydriasis, dry skin, quick pulse, dryness of the mouth, etc.

Crisis appeared in a further forty-eight hours, and there was no relapse. The culture of the sputum yielded the following organisms: *B. pfeiffer,* diplococcus of Fraenkel and a hæmolytic streptococcus. Beyond an extreme exhaustion and obstinate constipation, convalescence proceeded satisfactorily from the day of the crisis.

N.B.—The diagnosis was further confirmed by examination of a blood film, which revealed no polynuclear leucocytosis.

Curragh, Co. Kildare. I am, etc.,
August 15, 1921. J. E. H. GATT,
Major, Royal Army Medical Corps.

INDEX TO VOLUME XXXVII.

C.N. = Clinical and other Notes.
C.L. = Current Literature.

PAGE

Abacus in the laboratory, by Major F. W. W. Dawson .. .. C.N. 459

Abscess, hepatic, the treatment of, with emetine .. .. C.L. 317

Acromegaly in a young soldier, by Captain C. F. Burton .. .. .. C.N. 68

Alkaligenes, Bacillus fæcalis, a note on four cases of infection with, by Major D. P. Watson .. .. .. C.N. 143

Ambulances, field, the tactical handling of, by Colonel C. E. P. Fowler, letter from 78

Amœba dysentery in England .. C.L. 158

Amœbic dysentery, treatment of, with emetine bismuthous iodide, by Lieutenant-Colonel F. S. Irvine, letter from 391

Anæmia, splenic, in a child due to *Leishmania infantum*, by Major J. E. H. Gatt .. .. C.N. 142

Anderson, A. G., malaria in Macedonia 81

Anderson, Captain R. A., notes on the appointment of a Deputy Assistant Director of Medical Services (Sanitary) C.N. 211

Anoxæmia and its effects as seen in gas poisoning, by C. G. Douglas 216

Assistant Director of Medical Service in war time, by Lieutenant-Colonel W. F. Tyndall 23

Atropine in influenza-pneumonia, by Major J. E. H. Gatt, letter from .. 470

Bacillus enteritidis Gärtner, a case of continued fever due to, coincident with a gunshot wound of the chest, by Major R. G. S. Gregg and Captain P. Hayes C.N. 64

Bacillus fæcalis alkaligenes, a note on four cases of infection with, by Major D. P. Watson .. .. .. C.N. 143

Bainbridge, W. S., some factors in bone-repair 401

PAGE

Beamish, Captain D. W., iodine in the prophylaxis of influenza.. .. C.N. 381

Bilharziasis amongst British troops in Mesopotamia .. .. .. C.N. 376

Blackwater fever occurring in Mesopotamia, by Captain A. G. Harsant C.N. 384

Bladder, a villous growth of the, by Captain J. H. M. Frobisher .. C.N. 215

Bone-repair, some factors in, by W. S. Bainbridge.. 401

Boyd, Major J. E. M., entomological notes ... . C.N. 213

Briggs, H., physical exertion, fitness and breathing 278

Browne, Surgeon A. Lang, report on "Saddle Peak" (Andamans) 153

Browne, Colonel A. Lang, report on the Little Cocos 464

Burton, Captain C. F., notes on a case of acromegaly in a young soldier .. C.N 68

Castor oil, vaccination treated locally with, by Major E. H. Myles, letter from 397

Cocos, Little, report on the, by Colonel A. Lang Browne 464

CORRESPONDENCE—

- Atropine in influenza-pneumonia, by Major J. E. H. Gatt 470
- Eyesight, spectacles and rifle shooting, by Lieut.-Colonel L. Lloyd 393
- Import duties in India, by Major F. B. Lathbury 396
- Mumps, by Major J. E. H. Gatt .. 392
- Parasites (stage of *Spirochæta recurrentis*) in the liver of a fatal case of Mesopotamia relapsing fever, by Lieutenant-Colonel J. C. Kennedy 238
- Physical training in the Army, by Lieutenant - General Launcelotte Gubbins 398
- Physical training in the Army, by Major N. V. Lothian 239

PAGE

Relics of historic interest to the Royal Army Medical Corps, by Colonel H. A. Hinge 469
The tactical handling of field ambulances, by Colonel C. E. P. Fowler .. 78
Treatment of amœbic dysentery with emetine bismuthous iodide, by Lieutenant-Colonel F. S. Irvine .. 391
Treatment of malaria, by Colonel G. T. Rawnsley 79
Vaccination treated locally with castor oil, by Major E. H. Myles 397
Coscoroba bacilluria, a case of, by Captain T. O. Thompson .. .. .. C.N. 141

Dawson, Major F. W. W., the abacus in the laboratory .. .. .. C.N. 459
Disinfector, a dugout steam, by Lieutenant-Colonel P. H. Henderson C.N. 70
Douglas, C. G., anoxæmia and its effects as seen in gas poisoning 216
Dressing, shell, suggested improvement on the present, by Captain R. MacKinnon .. .. .. C.N. 462
Drought, C. W., relative incidence of injuries by gunshot wounds to peripheral nerves .. .. .. C.N. 386
Dugout steam disinfector, by Lieutenant-Colonel P. H. Henderson .. C.N. 70
Dysentery amœba in England .. C.L. 158
Dysentery Convalescent Depot, Wellington, by Captain T. O. Thompson C.N. 302

Ear, throat and nose, diseases of, by D. McKenzie, review of 237
ECHOES FROM THE PAST—
Report on the Little Cocos, Colonel A. Lang Browne 464
Emetine bismuthous iodide, treatment of amœbic dysentery with, by Lieutenant-Colonel F. S. Irvine, letter from .. 391
Emetine, the treatment of hepatic abscess with .. .. C.L. 317
Encephalitis lethargica, by Major A. T. Todd 72, 146
Enteric Convalescent Depot, Wellington, by Captain T. O. Thompson .. C.N. 302
Enteric fevers in the British Expeditionary Force, by Major-General Sir W. B. Leishman 1
Enteric group carriers, notes on a cutaneous reaction for the possible diagnosis of, by Captain T. O. Thompson .. .. C.N. 302

PAGE

Enteritidis Gärtner coincident with a gunshot wound of the chest, a case of continued fever due to bacillus, by Major R. G. S. Gregg and Captain P. Hayes .. .. C.N. 64
Entomological notes, by Major J. E. M. Boyd .. .. C.N. 213
Eyesight, spectacles and rifle shooting, by Lieutenant-Colonel L. Lloyd, letter from 393

Fæcalis alkaligenes bacillus, four cases of infection with, by Major D. P. Watson .. .. C.N. 143
Farming in Australia as a career for the public school boy, by Major-General Sir Robert Porter 208
Firth, Colonel Sir Robert, concerning a free-martin 62
Firth, Colonel Sir Robert, fragments 193, 432
Fowler, Colonel C. E. P., the tactical handling of field ambulances, letter from 78
Fragments, by Colonel Sir Robert Firth 193, 432
Frobisher, Captain J. H. M., a bulbous growth of the bladder .. .. C.N. 215
Frobisher, Captain J. H. M., case of stone in the ureter .. .. .. C.N. 459

Gärtner, enteritidis, bacillus, coincident with a gunshot wound of the chest, a case of, by Major R. G. S. Gregg and Captain P. Hayes .. .. C.N. 64
Gas poisoning, anoxæmia and its effects as seen in, by C. G. Douglas 216
Gatt, Major J. E. H., atropine in influenza-pneumonia, letter from 470
Gatt, Major J. E. H., mumps, letter from 392
Gatt, Major J. E. H., splenic anæmia in a child due to *Leishmania infantum* C.N. 142
Gout, by Ll. Jones Llewellyn, review of 159
Gregg, Major R. G. S., *Bacillus enteritidis Gärtner* coincident with a gunshot wound of the chest .. .. C.N. 64
Gubbins, Lieutenant-General Sir Launcelotte, physical training in the Army, letter from 398
Gunshot wounds to peripheral nerves, injuries by, by C. W. Drought.. C.N. 386

Hæmatophagy, the study of, by H. M. Woodcock 321, 418
Hæmorrhages, death from spontaneous, by Captain F. W. Harlow .. C.N. 312

PAGE

Harlow, Captain F. W., death from spontaneous hæmorrhages .. C.N. 312
Harsant, Captain A. G., bilharziasis amongst British troops in Mesopotamia .. .. C.N. 376
Harsant, Captain A. G., blackwater fever occurring in Mesopotamia .. C.N. 384
Hayes, Captain P., *Bacillus enteritidis Gärtner* coincident with a gunshot wound of the chest .. .. C.N. 64
Hele, T. S., malaria in Macedonia .. 81
Henderson, Lieutenant-Colonel P. H., a dugout steam disinfector .. C.N. 70
Hepatic abscess with emetine, the treatment of .. .. C.L. 317
Hiccough epidemic .. .. .. C.L. 156
Hinge, Colonel H. A., relics of historic interest to the Royal Army Medical Corps, letter from 469
Home, Major W. E., malaria and urobilinuria .. .. C.N. 463

Influenza, iodine in the prophylaxis of, by Captain D. W. Beamish .. C.N. 381
Influenza patients, experimental studies of the nasopharyngeal secretions from C.L. 317
Intestinal protozoa, notes on, by Margaret W. Jepps 366
Iodine in the prophylaxis of influenza, by Captain D. W. Beamish .. C.N. 381
Irvine, Lieutenant-Colonel F. S., treatment of amœbic dysentery with emetine bismuthous iodide, letter from.. .. 391

Jepps, Margaret W., notes on the intestinal protozoa of men at the War Hospital, Southampton 366

Kennedy, Lieutenant-Colonel J. C., parasites (stages of *Spirochæta recurrentis*) in the liver of a fatal case of Mesopotamia relapsing fever, letter from 238

Lambkin, Major E. C., the Wassermann reaction in the serum diagnosis of syphilis, report of the use of a substitute for the 161
Lathbury, Major F. B., import duties in India, letter from 396
LECTURE—
Anoxæmia and its effects as seen in gas poisoning, by C. G. Douglas 216
Encephalitis lethargica, by Major A. T. Todd 72, 146

PAGE

Leishman, Major-General Sir W. B., Enteric Fevers in the British Expeditionary Force 1
Leishmania infantum, a case of splenic anæmia in a child due to, by Major J. E. H. Gatt.. .. .. .. C.N. 142
Lethargica, encephalitis, by Major A. T. Todd 72, 146
Lloyd, Lieutenant-Colonel L., eyesight, spectacles and rifle shooting, letter from 393
Load carried by the soldier, by Major N. V. Lothian.. .. .. 241, 342, 448
Lothian, Major N. V., physical training in the Army, letter from 239
Lothian, Major N. V., the load carried by the soldier 241, 342, 448

Macedonia, malaria in, by C. M. Wenyon 81, 83, 172, 264, 352
Mackenzie, M. D., practical prevention of typhus fever and relapsing fever in Mesopotamia during the war .. 50, 127
Mackinnon, Captain R., suggested improvement on the present shell dressing .. .. C.N. 452
McLay, K., malaria in Macedonia .. 81
Malaria and urobilinuria, by Major W. E. Home .. .. C.N. 463
Malaria in Macedonia, by C. M. Wenyon, A. G. Anderson, K. McLay, T. S. Hele, J. Waterston .. 81, 83, 172, 264, 352
Malaria, treatment of, by Colonel G. T. Rawnsley, letter from 79
Malingering, the vision of the soldier, with special reference to, by W. Wallace 40, 109
Medical Services, Assistant Director of, of an army in war time, by Lieutenant-Colonel W. F. Tyndale 23
Medical Services, Deputy Assistant Director of (Sanitary), of a division in India, notes on the appointment of a, by Captain R. A. Anderson.. .. C.N. 211
Mesopotamia, the practical prevention of typhus fever and relapsing fever in, by M. D. Mackenzie 50, 127
Mumps, by Major J. E. H. Gatt, letter from 392
Myles, Major E. H., vaccination treated with castor oil, letter from 397

Nasopharyngeal secretions from influenza, experimental studies of the .. C.L. 317
Nervous system, diseases of the, by H. Campbell Thompson, review of.. .. 319

PAGE

Nervous system, the clinical examination of the, by G. H. Monrad-Krohn, review of 319
Nose, ear and throat, diseases of, by D. McKenzie, review of 237

Parasites in the liver of a fatal case of Mesopotamia relapsing fever, by Lieutenant-Colonel J. C. Kennedy, letter from 238
Peripheral nerves, injuries by gunshot wounds to, by C. W. Drought .. C.N. 386
Perry, Lieutenant-Colonel H. M., the Wassermann reaction in the serum diagnosis of syphilis, report on the use a substitute for 161
Physical exertion, fitness and breathing, by H. Briggs 278
Physical training in the Army, by Major N. V. Lothian, letter from 239
Physical training in the Army, by Lieutenant-General Sir Launcelotte Gubbins, letter from 398
Pneumonia .. .. C.L. 233
Poisoning, gas, anoxæmia and its effects as seen in, by C. G. Douglas 216
Porter, Major-General Sir Robert, farming in Australia as a career for the public school boy 208

Rawnsley, Colonel G. T., treatment of malaria, letter from 79
Relapsing fever, a fatal case of, by Lieutenant-Colonel J. C. Kennedy, letter from 238
Relapsing fever and typhus fever in Mesopotamia during the war, the practical prevention of, by M. D. Mackenzie 50, 127
Relics of historic interest to the Royal Army Medical Corps, by Colonel H A. Hinge, letter from 469
REPORT—
Saddle Peak (Andamans), by Surgeon A. Lang Browne 153
REVIEWS—
An account of the cavalry operations in Palestine and Syria, by Lieutenant-Colonel the Hon. R. M. Preston .. 468
A text-book of medical jurisprudence and toxicology, by J. Glaister .. 389
Auto-erotic phenomena in adolescence, by K. Menzies 319
Diseases of the nervous system, by H. Campbell Thompson 319
Diseases of the throat, nose and ear, by Dan McKenzie 237

PAGE

Essentials of medical electricity, by E. P. Cumberbatch 468
Gout, by Lieutenant Jones Llewellyn 159
Hygiene of communicable diseases, by F. M. Munson 467
Materia medica and pharmacy, by R. R. Bennett 469
On bone-formation, its relation to tension and pressure, by Dr. Mark Jansen 468
Orthopædic surgery of injuries.. .. 390
The birth and early days of our ambulance trains in France, 1914-1915, by Wagon-Lit 389
The clinical examination of diseases of the lungs, by Brockbank and Ramsbottom 468
The clinical examination of the nervous system, by G. H. Monrad-Krohn .. 319
The heart, old and new views, by H. L. Flint 390
What to do in case of poisoning, by W. Murrell 389
Rifle shooting and spectacles, by Lieutenant-Colonel L. Lloyd, letter from .. 393

Saddle Peak (Andamans), report on, by Surgeon A. Lang Browne 153
Shell dressing, suggested improvement on the present, by Captain R. Mackinnon C.N. 462
Soldier, the load carried by, Major N. V. Lothian.. .. .. .. 241, 342, 448
Soldier, the vision of, with special reference to malingering, by W. Wallace 40, 109
Spectacles and rifle shooting, by Lieutenant-Colonel L. Lloyd, letter from.. 393
Splenic anæmia in a child due to *Leishmania infantum*, by Major J. E. H. Gatt .. .. C.N. 142
Stone in the ureter, case of, by Captain J. H. M. Frobisher .. .. C.N. 459

Thompson, Captain T. O., a case of coscoroba bacilluria .. .. C.N. 141
Thompson, Captain T. O., cutaneous reaction for the possible diagnosis of enteric group carriers .. .. C.N. 302
Throat, nose and ear, diseases of the, by Dan McKenzie, review of 237
Todd, Major A. T., *Encephalitis lethargica* 72, 146
Tyndale, Lieutenant-Colonel W. F., an Assistant Director of Medical Service of an army in war time 23

PAGE

Typhus fever and relapsing fever in Mesopotamia during the war, the practical prevention of, by M. D. Mackenzie 50, 127

Ureter, a case of stone in the, by Captain J. H. M. Frobisher .. .. C.N. 459

Urobilinuria and malaria, by Major W. E. Home .. .. .. C.N. 463

Vaccination treated locally with castor oil, by Major E. H. Myles, letter from 397

Villous growth of the bladder, a, by Captain J. H. M. Frobisher .. C.N. 215

Vision of the soldier, with special reference to malingering, by W. Wallace 40, 109

Wallace, W., the vision of the soldier, with special reference to malingering 40, 109

PAGE

Wassermann reaction in the serum diagnosis of syphilis, report on the use of a substitute for the, by Lieutenant-Colonel H. M. Perry and Major E. C. Lambkin 161

Waterston, J., malaria in Macedonia .. 81

Watson, Major D. P., four cases of infection with bacillus fæcalis alkaligenes .. .. C.N. 143

Wenyon, C. M., malaria in Macedonia, the incidence and ætiology of .. 81, 83, 172, 264, 352

Woodcock, H. M., hæmatophagy, the study of 321, 418

Yellow fever, experimental transmission of .. .. C.L. 159

Notices.

EDITORIAL NOTICES.

The Editor will be glad to receive original communications upon professional subjects, travel, and personal experiences, etc. He will also be glad to receive items of news and information regarding matters of interest to the Corps from the various garrisons, districts, and commands at home and abroad.

All such Communications or Articles accepted and published in the "Journal of the Royal Army Medical Corps" will (unless the Author notified at the time of submission that he reserves the copyright of the Article to himself) become the property of the Library and Journal Committee, who will exercise full copyright powers concerning such Articles.

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